SCIENCE.

FRIDAY, SEPTEMBER 7, 1883.

FRANCIS MAITLAND BALFOUR.

About a year ago came the sad news of the sudden death of Professor Balfour of Cambridge. If the loss was felt less severely in this country than in England, it was only because he had fewer personal friends here; and to fully understand his worth one must have known and talked with him. It is true that it required no unusual insight to read the fine qualities of the man in his writings; but none save those who knew him could appreciate his remarkable personal attractiveness. Not the least part of the wonderful work of his short life was that which he accomplished as a teacher: here as everywhere, his personal influence had a large share; and a sketch of Balfour's scientific work would be incomplete without a recognition of the bearing which his noble character had upon it.

Yours ever
F. M. Balfour

The meeting of leading biologists in October last, to found the Memorial studentship, was remarkable in many ways: rarely have been heard such words of admiration and love for one man as were then expressed for Balfour. Many spoke at length of the debt Cambridge owed him. It may be said that he divided with Foster the honor of giving the great impetus to the biological movement in the English universities. What Huxley had done for Foster, the latter did for Balfour, giving him the first hearty encouragement and support; together they raised biology from the third to the level of the first rank of studies at Cambridge, equalling that held by mathematics. Oxford soon followed this important movement, trying to secure Balfour for the professorship left vacant by the death of Rolleston. His connection with natural science at Cambridge was described in warm language by Foster, his teacher, and by Sedgwick, one of his pupils: he advanced morphology there by his brilliant success in teaching and in research.

In teaching he combined manly force with a delicate regard for the feelings of his pupils. From the writer's personal impressions of him as a lecturer, he did not aim at eloquence, but to be understood in every step; rarely looking at his hearers, he spoke rapidly and with intense earnestness, crowding a vast deal into the hour. The main qualities of his character shone forth in his lectures, — energy, which he infused into his hearers; truthfulness, which soon gave implicit confidence in his statements; modesty and sympathy, which inspired effort and free exchange of thought.

Balfour's love of truth came constantly into play in his laboratory instruction. While looking over a student's shoulder, he would sometimes say with a laugh, "You must interpret that specimen with the eye of faith;" but this was very far from being a serious injunction, for he exacted of his students the greatest caution in the progress of their microscopic work. However tempting a certain interpretation of a specimen might be, Balfour never accepted it until it rested on the clearest evidence. An instance of this sort is recalled by the writer, which related to the much disputed origin of a well-known embryonic structure. A number of sections had been prepared, seeming to confirm the view which Balfour himself had advocated some time before; it required considerable self-control not to attach a somewhat forced meaning to them: this was, however, forbidden; and it was not until several days afterwards that fresh sections established the fact beyond question.

To Foster, Balfour repaid his student-debt by extending, in turn, continued encouragement to others. He did not fear, as many great teachers have, that joint labor with his juniors would derogate from his reputation: his joint articles are numerous; he was zealous to recognize research done by his pupils, seeming to be prouder of this than of his own work. Nothing could be more stimulating to the young men about him, still distrustful of their powers, than this generous co-operation. Is it surprising, then, that the voluntary attendance upon his lectures increased in seven years from ten to ninety, and that at the time of his death twenty students were engaged in difficult research in his laboratory? Only those who are familiar by experience with the few incentives among younger students to the study of biology can appreciate what these numbers mean.

We need not attempt to give a full list of Balfour's writings. They began in 1873, his twenty-first year, with a few short papers appearing over Foster's name and his own in the *Quarterly journal of microscopical science*: they terminated nine years later, with his fine work upon Peripatus, published posthumously in the same journal, and of which a full abstract will be found farther on. His extensive intermediate works, the Elasmobranch fishes and Comparative embryology, are universally known.

From the first he devoted himself to embryology. While this, as among the youngest of the biological sciences, admits of rapid work, it is far from admitting rapid generalization. No other branch of morphology requires more painstaking; the very materials one has to study are minute and indefinite; and two minds will often place different constructions upon the same specimen. There is abundant opportunity for scientific guesswork, with the feeling of security that disproval will be difficult. Balfour understood the real value of guessing at truth, but he always made it very clear to the reader when he was so doing; his hypotheses were accompanied by definite statements, in which the reasons *pro* and *con* were set forth in all impartiality to each. Herein lies a chief charm and merit of his work, its brilliant suggestiveness, side by side but never in confusion with well-established facts. Every chapter contains half a dozen invitations to other investigators to prove or disprove certain provisional statements. Vast as is the information contained in his Comparative embryology, Balfour himself appreciated, that, as far as mere facts went, the first volume would be somewhat out of date before the second was in press. Not so, however,

with his masterly discussions of these facts, which are found on every page, and the value of which, to embryologists, cannot be estimated. Moreover, to his authorship is largely due the rapidly spreading interest in embryology in England and America, — a branch of science, it will be remembered, which had previously been mostly in German hands.

One frequently heard from him his own very modest opinion of his work; this was not at all inconsistent with striking independence and originality of thought, and adherence to his convictions. His modesty added more to the recognition of his genius than any assertions of his own could have done. Many were pressing forward to assert his claims, and honors were fast showered upon him in England and abroad. He was admired and beloved by all who knew him. In scientific discussion he had the rare quality, which Richard Cobden is said to have possessed, of remaining on the pleasantest personal terms with his opponents.

His energy in all matters was great, and his power of writing was unusually rapid; but, advised by kind friends, he rarely overtaxed his strength, which was limited. He spent most of his evenings with his friends, throwing off from his mind the labors of the day, and talking vivaciously upon the topics of the times. When the first volume of Comparative embryology was being written, he generally worked but five hours daily, giving much time to physical exercise, bicycling, or tennis, into which he entered with all the enthusiasm of his nature. He was courageous, but not reckless; and nothing in his previous life would lead us to suppose that the mountain climb which proved fatal was undertaken in a foolhardy spirit.

Balfour in a few years accomplished the work of a lifetime. His influence was and is twofold, — first, upon those with whom he came in personal contact, especially his scientific associates and students (an influence which cannot fail to endure, well expressed by Professor Kitchen Parker: "I feel that his presence is still with me; I cannot lose the sense of his presence"); and, secondly, the influence of his scientific work, which for genius, breadth, and truth, can never be surpassed. May the splendid memorial which has been raised for him perpetuate his noble example as a teacher and man of science! HENRY F. OSBORN.

THE INTELLIGENCE OF BIRDS.

HAVING met with many instances wherein birds have shown considerable ingenuity in overcoming the ill results of accidents to their nests, such as often arise during violent storms, it occurred to me, at the outset of the bird-nesting season of the present year, to endeavor to test their intellectual powers generally, by a series of simple experiments, hoping thereby to be able to determine to what extent birds exercise their reasoning faculties.

My experiments, and the inferences I drew, are as follows: —

Noting the material being gathered for the nest, partially constructed, of a chipping-sparrow (Spizella socialis), I placed a small quantity of the same in a conspicuous position near the nest. It was seen by the sparrows, and examined, but none was removed. I placed a portion of it upon the margin of the unfinished nest: it was promptly removed by the male bird, who used only such materials as were brought to him by his mate. The following day the task of lining the nest with hair was commenced. I placed a quantity of this material on a branch near by, but it was passed unnoticed. I next placed a few hairs on the margin of the nest: they were promptly removed. On replacing many of these in the nest, the entire lining was thrown out. I replaced it, and the nest was abandoned.

A week later, finding another nest with three eggs, I added a few white cat-hairs to the lining: these were removed. Others of dark colors were added: they, also, were removed. I replaced both dark and white hairs: the eggs were broken, and the nest abandoned.

Four eggs found in a third nest were removed without touching the nest, a wooden spoon whittled for the purpose being used. In three days the female commenced laying again: four days later three eggs had been laid. Replaced the four I had removed: they were promptly thrown from the nest. I then removed the nest, and, substituting another, carefully replaced the eggs without handling them. After what appeared to be a serious consultation, the new nest was accepted.

These birds suffered no further annoyance, and reared their brood without mishap.

Why should not these have utilized the material for their nest which I offered, rather than gather similar stuff from distant points? They could not have been frightened by any odor attached to the material through handling, as I was careful not to touch a particle of it, using a pair of wooden tweezers in every case. Neither did they see me carrying any thing to or from their nests. As these, in all cases, were nearly or quite completed, the birds had necessarily become thoroughly familiar with the surroundings, and doubtless recognized the fact that these offered twigs and the hair had suddenly appeared in, to them, some unexplained manner, and the mystery surrounding it made them suspicious. Suspicion, I suggest, is a complicated mental effort. Again: the sparrows were sorely perplexed when a nest not of their building, but of the same character, was substituted for their own. Here, these birds exhibited fear; but finally the maternal instinct overcame the timidity of the female, and she resolved to brave the danger or solve the mystery, and cared for her eggs as usual. The male bird kept aloof for several days, I think; but of this I am not positive. These sparrows were moved by conflicting emotions, — evidence, I think, of an advanced degree of intelligence.

Another series of experiments were as follows: finding a nest of the summer warbler (Dendroeca aestiva) in a low alder, the foliage of which was about one-third grown, I girdled the supporting growths a few inches below the nest. The leaf-buds withered, and the nest, which under ordinary circumstances would have been quite concealed from view by the full-grown leaves, was now exposed. The nest was abandoned.

The next girdling experiment was made on the nest of a white-eyed vireo (Vireo noveboracencis) found attached to a low limb of a small beech. The leaves quickly shrivelled, and the nest, although just finished, was abandoned.

A second experiment of the same sort was tried, with identical result.

A nest of the summer warbler was found in a low shrub, containing young birds, and the supporting branches girdled. The leaves withered and fell, exposing the nest to full view. The parent birds remained, and successfully reared their brood.

In these cases we have evidence of mental operations of a more complicated character than any exhibited by the sparrows. It is evident, that in every case, these birds, in selecting the position for their nests, knew that the growth of the foliage would afford a desirable, if not necessary, protection to them. Finding that the growth of the foliage had been checked, that the little shelter at first afforded was daily growing less, they foresaw that the nests, under these circumstances, would be too much exposed to be safe from molestation, and they were abandoned, even after a full complement of eggs had been laid. Can we explain this by any other means than by using that very suggestive term 'foresight'? But mark: when the same circumstance occurred after the young had appeared, the claims of the brood upon the parents were too strong to be overcome, and the danger of occupying an exposed nest was readily braved.

Experiments of another character were as follows: I placed a series of short pieces of woollen yarn, fastened together at one end, near the tree containing a partially constructed nest of a Baltimore oriole (Icterus Baltimore). These yarns were red, yellow, purple, green, and gray. An equal number of strands of each color were thus offered to the orioles as building-materials. I purposely placed the red and yellow strands on the outside of the tassel-shaped mass, so that these would be first taken, if the color was not objectionable. To my complete surprise, the gray strands only were taken, until the nest was nearly finished, when a few of the purple and blue yarns were used. Not a red, yellow, or green strand was disturbed. Here we have an instance of the exercise of choice, on the part of a bird, which is full of interest. The woollen threads being otherwise identical, it was the color only that influenced the choice of the birds: they realized that the red or yellow yarns would render the nest conspicuous, although well protected by the foliage of the branch to which it was attached. Why the green threads were not taken I cannot imagine. As a result of this experiment, I anticipated that the orioles would reserve the brightly colored yarns for the lining of the nest, and the gray and green for the exterior. This was a result obtained two years ago, when I tried a similar experiment; but the use of red yarn as a lining may have been merely accidental.

Out of mere curiosity, for I could not anticipate what might be the result, I made a few transfers of the eggs of one species into the nest of another bird. The results were not, however, particularly suggestive. I placed the eggs of a cat-bird (Mimus carolinensis) in the nest of a song-thrush (Turdus mustelinus),

and *vice versa*. The eggs of the former are dark green; of the latter, light blue. No act indicative of recognition of the change was observed. I placed eggs of the song-sparrow (Melospiza melodia) in the nest of a pee-wee (Sayornis fuscus), and *vice versa*. The fly-catchers rejected the eggs of the sparrow; but the latter accepted the situation, although disturbed by it. Many other changes were made, with similar results; and I concluded, that, unless the eggs were greatly different in size and color, about one-half would be accepted; but, when a single egg was placed in the nest of another bird, it was destroyed in nearly every case. This I found to be true, even when I tested such birds as are subjected to the annoyance of the cowpen bird's egg being deposited in their nests. I was surprised at this result, and am led to believe that large numbers of the eggs of this bird are destroyed. It is well known that our summer warbler frequently outwits the cowpen bird by building a new nest directly above the old, — a two-story nest, in fact, — and leaves the egg that has been left to her care to rot in the basement, while she rears her young on the floor above. It will be seen that from these experiments no very positive results were obtained. I did note, however, that, where the change was accepted, it was not because it passed unnoticed, but was submitted to, notwithstanding the evidences of much misgiving on the part of the birds. In one case, the nest was practically deserted for twenty-four hours, and the eggs were chilled in consequence. The birds sat upon them for five days, when, as they did not hatch, the nest was abandoned. In previous years I have made these changes occasionally with success, but was not able to determine that the young were recognized as not the offspring of the parent birds. In such cases the young were tended with the usual care up to the time for leaving the nest. This may possibly be indicative of stupidity. It appeared so to me at the time; but I am now disposed to see in it an indication that the maternal instincts here, as in other cases I have mentioned, overcame all other feelings, and that the fact was accepted by the birds with as good grace as they could command.

The co-operation of birds, when constructing their nests, is a subject that demands a good deal of close attention, and is one surely worthy of more systematic observation than has as yet been given it. The many ways in which birds assist each other in nest-building offer, perhaps, the clearest evidence that they have a very intelligent notion of what they are doing, or propose to do. I feel warranted at the outset in making the somewhat startling assertion, that the choice of location for a nest is made only after protracted joint examination of suitable sites, and is the choice of both birds. I doubt if it ever happens that one of a pair of birds 'gives in' to its mate. Certainly such a thing as madame giving up to her lord is unknown in the bird-world. My impression is, that the female birds of every species are exacting, obstinate, and tyrannical. I have seen marked instances of this among house-wrens, pee-wees, and even known a cooing turtle-dove to exhibit unmistakable evidences of a quick temper. These may seem to be trivial matters, and not within the range of the scientific study of animal intelligence; but it is an error to look upon such proofs of individuality in this light: they are among the most convincing evidences of a high degree of intelligence. If a hundred or more nests of the same species of birds are carefully compared, it will be found that there is a considerable range of variation in their construction, and a varying degree of merit in the skill shown by the builders. Is not this evidence of different degrees of mental strength occurring among birds of the same species?

But to return to the subject of co-operation in nest-building. I have found, that where very long, fibrous materials are used, as in the case of the globular nests of the marsh-wrens, the birds work together in weaving the long grasses that form the exterior. I have seen one of these birds adjusting one end of a long blade of rush-grass, while its mate held the other end, until the former had completed its task to its satisfaction. It was evident that the weight of the ribbon-like growth that the bird was using, quite a metre in length, was too heavy to be moved to and fro, and at the same time prevented from slipping from the unfinished nest. Only by assistance could such materials be utilized, and only by intelligent joint labor could these little birds build such large and complete globular nests. Many birds, too, have been known to jointly carry away a long string or piece of muslin too heavy or cumbersome for either one to move. Again: materials are often brought by one of a pair of birds to a nest which the other considers unsuitable, and fierce quarrels often arise from this circumstance. In such cases we have instances of a difference of opinion among birds, which is a marked indication of mental activity.

CHARLES C. ABBOTT, M.D.

THE IGLOO OF THE INNUIT.[1] — IV.

The interior of an igloo can be best understood by reference to the diagrams. The one, fig. 1, is a vertical section through the entrance; and the other, fig. 2, a ground-plan. Directly opposite the entrance is raised a platform of solid snow, eighteen inches to two feet

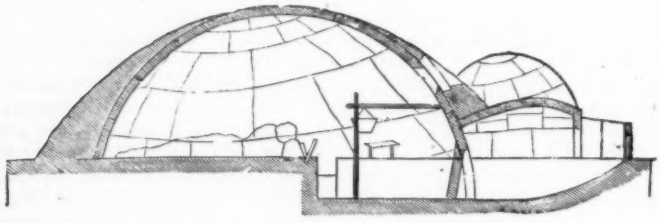


Fig. 1.

in height, which takes up about two-thirds of the floor; and on this are spread the reindeer-skins which make the bed. Sometimes, if the party be large and but one igloo built, there are two of these snow-beds, separated by a narrow aisle running from the entrance; the persons then sleeping at right angles to the positions shown in the illustration. But such large igloos are rare, unless of a permanent or semi-permanent character. On an extension of the platform forward, on the woman's side, is placed the stone lamp; and here the food is cooked, and the native skin clothes are dried. The height of this platform or snow-bed is nearly always above the top of the low door; for the Innuits are instinctively masters of the simple laws of pneumatics, and try to keep the snow-bed as high as possible to reach the upper or warmer strata of air, especially to keep higher than the cold air, which can come in through the open door. The height varies with the permanency of the abode, the temperature, and with the tribe. If very cold, or if intending to occupy the igloo for some time, the beds are made higher than they would be otherwise. The Netschilluks and Kinnepetoo always make much higher beds than the Iwilliks or Iglooliks. There is also much variation in the flatness of the dome; those of the former tribes, especially the Netschilluks, being very flat. This, with their high beds, makes the space between them very small; but in compensation their igloos are the warmest and most comfortable in the whole arctic region. These Netschilluks (in and around King William's Land) nearly always have to jump out in front of their beds to get standing-room to dress in, although all Innuits are adepts in the art of putting on the most intricate clothing in the smallest space conceivable.

The Kinnepetoo Innuits (around Chesterfield Inlet, especially north of it) use few or no lamps to warm their snow-huts, and, despite the high beds and low roofs, they are cold, cheerless, and uncomfortable beyond measure. These Innuits are essentially reindeer killers and eaters, and lay in an insignificant stock of seal-oil to burn in their lamps. Walrus-killing is unknown to them. For light they use a piece of rendered reindeer suet, laid beside a piece of lighted moss, all being on a large flat stone. The light of the stone lamp in all igloos where it is used is sufficient for all purposes of sewing and repairing. It is certainly

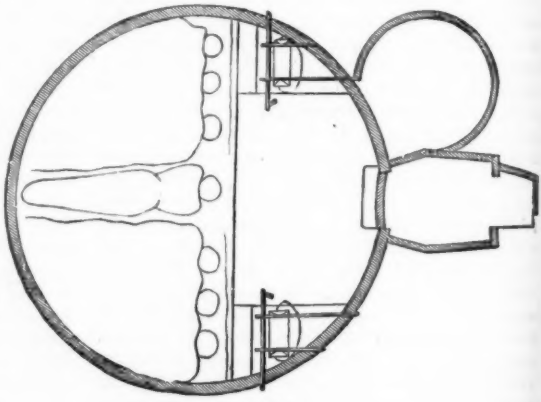


Fig. 2.

equal to the light from three or four kerosene-lamps, and, with the white snow-walls, gives ample illumination.

The Oo-quee-sik Salik Innuits (around the mouth of Back's River), who are salmon-eaters, are another tribe that dispense with warming the snow-houses for want of oil; and this with their very poor stock of clothing, they being

[1] Continued from No. 30.

almost constantly in rags, makes them a most forlorn, uncomfortable-looking, and dejected lot of human beings. The powers of these two tribes to withstand the cold seem almost phenomenal.

The flatness of the domes, however, is not wholly a tribal peculiarity, but is also a function of the season of the year. In the wintertime, when the snow is hard and compact, the roof can be made much flatter than in the spring, when the warm, sunny days bring on a thaw, and threaten to tumble it in. At such times it is made very peaked, to gain strength for its weakest points, the inclining blocks.

The Iwilliks and Iglooliks (among the estuaries of North Hudson's Bay) have ample supplies of whale, seal, and walrus oil, and, despite their higher roofs, have very comfortable houses in the way of warmth, while they exceed all others in roominess, and ease and comfort in dressing and undressing.

The heated air, of course, rises to the top; and, should it grow too warm inside, this heat soon cuts its way through the joints of the top blocks, and enough fresh air enters to quickly reduce the temperature below freezing again, especially if it be very cold on the outside. Sometimes this ascending heat makes so much impression on the edges of the top blocks that they commence to thaw and drip in an annoying manner. This is always remedied by taking a handful or a small block of snow from the floor, where the temperature is very low, and applying it to the dripping spot, where it freezes immediately, and, like a sponge, absorbs the drippings. These little pests have to be watched closely, however: for when they are saturated with water, and thawed from their frozen fastenings, they will come down like a slushy ball of lead; and it seems as if they would defy all the laws of gravity to get down a person's back, or hit a sleeper in the face. I once had a large one fall in a pint cup full of hot reindeer-soup just as I had it near my nose, blowing it to hurry up the meal and get away from a delayed camp.

Small store-igloos are built outside to hold the bulky material, and often connect with the main igloo or its entrance, if their contents are needed from time to time.

Where several families, generally related, build a family igloo, it is done by making a large central one, without bed-platforms or other impediments to roominess; and around this are built the smaller family igloos, — two, three, or even a half-dozen, — connecting with the central one by high groined arches that will generally allow of passing from one to the other without stooping; and conversation can be readily carried on between them, these smaller igloos being more like radiating alcoves than separate structures. Then the entrance to the main part is made very long (fifteen or twenty feet), and its outer end is changed from time to time to face *away* from the wind, if it be at all strong. The usual entrance is so low that one always has to enter on his hands and knees; but in these family igloos the greater part one can accomplish by stooping considerably. There is always a crowd of hungry dogs ready to take advantage of a person's entering to crowd in close behind, so as to steal a stray piece of blubber from the lamp-platform or floor. At all other times two or three of their heads can be seen closing the entrance, waiting a good opportunity for a dash. The matron of the house, sitting *à la Turc* on the edge of the bed, keeps a good stout club convenient, and whacks them over the nose whenever they make an unusually impudent intrusion. At night-times, and during cold, windy weather, the more belligerent of these camels of the cold monopolize the entrance for sleeping-apartments; but they generally manage to get into some sort of fight, breaking in the door, and the master then arises and vacates these canine compartments with the butt-end of a whip or a sledge-slat, and they remain quiet for the rest of the night.

The temperature inside ranges from freezing (above which, of course, it cannot ascend) to about ten to twenty degrees below. Late in the winter, when all have inured themselves to the cold, the same tribe will keep their houses much colder with the same apparent comfort. At these temperatures one feels very warm after coming in from the outside. The outer clothes are taken off, and even baths are indulged in; the little children, *stark naked*, playing on the reindeer-skins of the bed with the little puppies and toy harness. Those tribes that do not use oil-lamps are, of course, much colder in their houses, having only the warmth of the body and a few lights, with occasionally some cooking from the lamps; yet I do not think it ever gets below zero. Even in these igloos I have known a Kinnepetoo to take a reindeer-skin that had been soaked to rid it of hair, and that was apparently frozen as solid as boiler plate iron, and, putting it under his coat against the bare skin, hold it there not only until it was thawed out, but also until it was dry, and fit to be used for a drumhead for their superstitious rites. Juggernaut could show no greater devotees among

his followers. Such are the iron Innuits of the unwarmed igloos of the Arctic.

A recently constructed igloo is more comfortable than one long used, the alternating heat and cold of the day and night soon converting the latter into a translucent mass of ice, that becomes uncomfortably chilly on a cold night; besides, the steam from the cooking and the moisture from the breath congeal upon the roof, and, in the course of ten or twelve days, become so thick as to form a base for a constant liliputian snow-storm, which is disagreeable beyond measure. One of the most conspicuous comforts of arctic travelling is the constant changing of igloos.

(To be continued.)

BALFOUR'S LAST RESEARCHES ON PERIPATUS.

At the time of his death, the late lamented Prof. F. M. Balfour was engaged upon an investigation of the anatomy and development of Peripatus, the lowest known form of Tracheata (insects). Unfortunately, he left his work far from complete; but two friends, Mr. Sedgwick and Professor Moseley, both thoroughly competent, have undertaken and completed the grateful task of editing what could be gathered from Balfour's material. We have, however, hardly more than a descriptive account of the anatomy and development of the animal. We miss the fruitful thought with which Balfour enriched his writings before committing them to the press.

The article is published in the April number of the *Quarterly journal of microscopical science*, and is accompanied by numerous beautiful plates. A portion of these were drawn by Miss Balfour. Their excellence graces this quiet expression of a sister's close relation to a gifted brother.

Balfour's investigations were directed especially upon Peripatus capensis. The memoir opens with a careful description of the external characters of the species. The account of the legs is the first satisfactory one published. The number of legs is variable, but usually there are seventeen pairs. Each leg has the form of a cone, with a pair of claws at the apex: it bears a succession of rings of papillae, but towards the tip the papillae in part fuse together to form three ventrally placed pads. The foot is distinct, being separated by a constriction from the upper part of the limb, and has several pads upon its ventral surface, and bears the two conical recurved claws. On the middle of the ventral line of junction of the leg with the body lies the opening of the segmental organs. The disposition of this opening on the fourth and fifth legs is slightly different. The last leg has a papilla with a slit-like gland opening at its apex. The gland itself is large, and runs far forward, and is probably a modified crural gland.

Part II. is a monograph of the internal anatomy. In the *alimentary canal*, a nearly straight tube slightly longer than the body, five parts may be distinguished. 1. The buccal cavity. Its opening is surrounded by a tumid lip, covered by a soft skin raised into papilliform ridges. Attached to the median dorsal wall of the cavity is a muscular protuberance (tongue), covered by the oral epithelium, and furnished with organs of special sense, like those in the skin, and with chitinous teeth. On each side of the tongue is placed the jaw, with recurved chitinous teeth. The jaws are, no doubt, modified limbs: their structure and action are minutely described. The salivary glands open into the buccal cavity by a short common duct, are variable in length, but stretch usually two-thirds the length of the body. They consist of two parts: the first runs backward as a wide, straight tube; the second runs forward and upward, is small in diameter, and apparently branching in the figures, though the fact is not mentioned in the text. The anterior end of the first part serves as a duct, and is lined by a cubical-celled epithelium; while the rest of the same part is

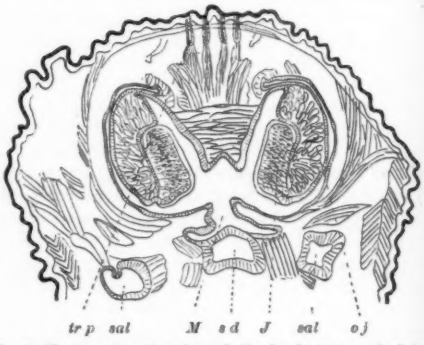


Fig. 1. Horizontal section through the head: *tr p*, tracheal pit; *sal*, salivary gland; *M*, mouth; *s d*, common salivary duct; *J*, jaw; *o j*, outer jaw, or muscular portion; between the two jaws lies the section of the tongue.

glandular, and lined by very elongated epithelial cells with their nuclei at their bases. 2. The pharynx is a highly muscular tube, with a triangular lumen, which extends from the mouth to about half way between the first and second pair of legs. (It appears to me that the author is in error when he states that such a structure is not characteristic of insects.) 3. The oesophagus, on the dorsal wall of which occurs the junction of the two sympathetic nerves. 4. The stomach, by far the largest part of the alimentary tract, has its walls irregularly, not segmentally, folded. The walls themselves are composed principally by the internal epithelium, the cells of which are elongated, fibre-like, with their nuclei about one-fourth of the way from the base; and around their bases are short cells irregularly scattered, and having round nuclei. 5. The short rectum is chiefly remarkable because the circular muscular layer is *outside* the internal layer formed of isolated longitudinal bands.

The *nervous system* is particularly interesting; for it consists of two ventral cords united by numerous transverse bands, and having an enlargement corre-

sponding to each leg. The cords are united in front, above the oesophagus, to form the cephalic ganglia, and are also united behind over the anus. The arrangement of the commissure and nerves of the ventral cords is minutely described. The supra-oesophageal ganglia give origin to the immense antennary nerves, and a few small epidermal nerves; laterally, one-third of the way back, the optic nerves, and two pairs of smaller nerves near the optic; still farther back, a large median nerve from the dorsal surface;

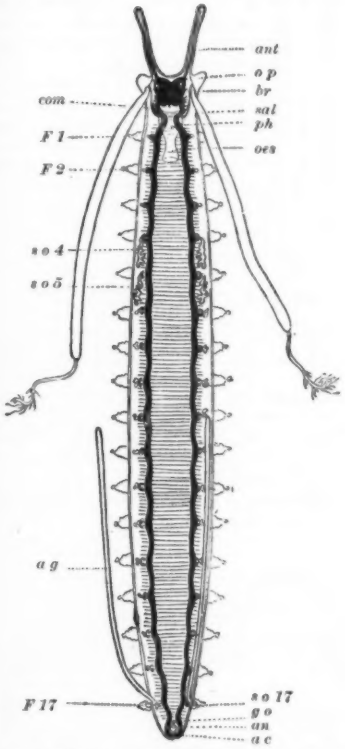


FIG. 2. General anatomy; the digestive tract is supposed to be excised; the nervous system is represented in black: *ant*, antenna; *o p*, oral papilla; *br*, brain; *sal*, salivary gland; *ph*, pharynx; *oes*, oesophagus; *com*, commissures; *F 1*, *F 2*, *F 17*, feet; *s o 4*, *s o 5*, *s o 17*, segmental organs; *a g*, accessory gland; *g o*, genital opening; *an*, anus; *a c*, anal commissure.

from the ventral surface, the sympathetic nerves, which follow the grooves of the pharynx, and unite upon the dorsal wall of the oesophagus. The ganglion-cells are confined, for the most part, to the surface in the supra-oesophageal ganglia, and to the ventral layer in the longitudinal cords. On the under side of each lobe of the brain is a conical protuberance of ganglion-cells, which Grube regards as an organ of hearing; but Balfour questions that interpretation.

The *skin* resembles that of other insects. The cuticle is thin, and forms a separate conical cap over each cell. The surface of the cuticle is dotted over with minute spinous tubercles. Scattered over the skin are organs of special sense, which I think resemble the olfactory organs of insects; but Balfour regards them as tactile. Each is a broad, conical, cuticular spine supported by large specialized sensory cells.

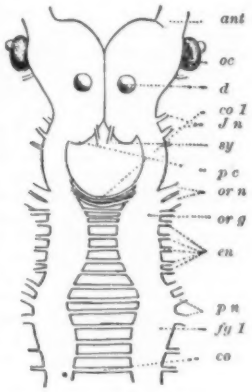


FIG. 3. Anterior portion of nervous system: *ant*, antennal nerve; *oc*, eye; *d*, ventral appendages; *co 1*, first commissure; *J n*, nerves of the jaw; *sy*, sympathetic nerves; *p c*, posterior lobe of brain; *or n*, nerves of the oral papillae; *or g*, ganglion of oral nerves; *en*, lateral nerves of ventral chords; *p n*, pedal nerves; *fg 1*, enlargement corresponding to pedal nerves; co, commissure.

The *tracheae* arise from openings between the ridges of the skin. Each aperture leads into a pit formed by the invaginated skin; and from the bottom thereof springs a bunch of fine tracheal tubes, which display large adherent nuclei on their walls, and transverse lines indicating the presence of a spiral fibre. The openings form two rows (subdorsal) on the back, and two rows on either side of the median ventral line; they are also found on the feet, around the bases of the feet, and on the head.

The *muscles* of the jaws are alone striated: all others are unstriated. The muscles of the body form an external double layer of circular fibres, an inner layer of longitudinal muscles forming five bands (one

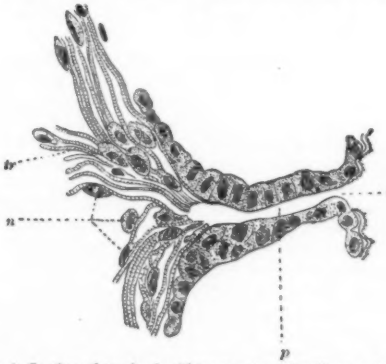


FIG. 4. Section of tracheal orifice: *o*, external orifice; *p*, pit; *tr*, tracheae; *n*, tracheal nuclei.

being median and ventral), and vertical septa of transverse fibres (one septum on each side of the alimentary canal): so that the body-cavity is divided into three regions, — a median, containing the alimentary tract, slime-glands, etc.; and two lateral, containing

the nervous system, salivary glands, segmental organs, etc.

The *vascular system* is imperfectly known. Balfour describes a dorsal tube without apparent muscular walls as the probable representative of the heart, and mentions a less distinct ventral vessel. (Cf. note.)

The *segmental organs*, which were first recognized by Balfour,[1] conform to the structures designated by the same name in annelids. They consist of: 1°. a vesicular portion opening to the exterior; 2°. a coiled portion, which is again subdivided into several sections; 3°. a terminal section ending by a somewhat enlarged opening into the lateral compartment of the body-cavity. The first two pairs, corresponding to the fourth and fifth legs, differ somewhat from the rest, which are all similarly constructed. They are lined by an epithelium, which varies in character in the different parts of the organs: in the first portion, the cells are large, flattened, and have large protuberant nuclei; the second portion has a columnar epithelium in its outer part, in which, further, two regions may

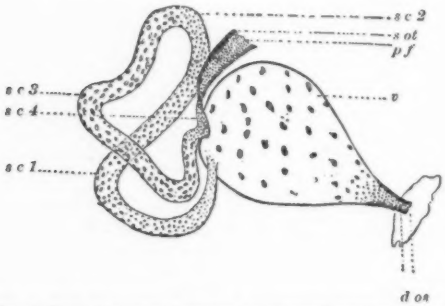


Fig. 5. Part of segmental organ: *o s*, external opening of the segmental organ; *d*, terminal portion of duct; *v*, vesicle; *s c*, *1*, *2*, *3*, *4*, successive portions of segmental canal: *p f*, internal opening; *s ot*, terminal portion.

be distinguished histologically; a third region within this outer part has large, flat, granular cells, with disk-like nucleolated nuclei; while a fourth region, the innermost of the middle portion again, has a lining of small columnar cells. The inner portion has a thick columnar epithelium crowded with oval nuclei, and opens with reflected lips into the body-cavity.

The *generative organs* are briefly described by the editors, who do not, however, deal with their histology. The male organs consist of a pair of testes, a pair of prostates, and vasa deferentia and accessory glandular tubules. The female organs consist of a median unpaired ovary and a pair of oviducts, which are dilated for a great part of their course to perform a uterine function, and which open behind into a common vestibule communicating directly with the exterior. In all the legs except the first there are glandular bodies. The large accessory gland opening in the last leg of the male is probably a modification of one of the series for which the name 'crural glands' is proposed.

Part III., also entirely written by the editors, treats of the development. This contains illustrations, serving to accompany the notice published in the Royal society's proceedings (Science, i. **453**); certain requisite explanations are added; then follow descriptions and figures of older embryos than had been previously described by Balfour. Special attention is called to the following more important facts:—

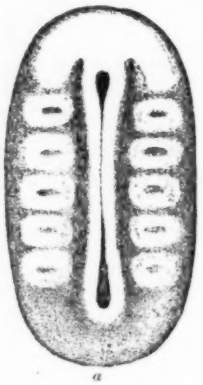


Fig. 6. Embryo, 'stage C,' with five somites: *a*, anal (?) end. The lips of the blastopore have united in the middle.

"1. The greater part of the mesoblast is developed from the walls of the archenteron.

"2. The embryonic mouth and anus are derived from the respective ends of the original blastopore, the middle part of the blastopore closing up.

"3. The embryonic mouth almost certainly becomes the adult mouth; i.e., the aperture leading from the buccal cavity into the pharynx, the two being in the same position. The embryonic anus is in front of the position of the adult anus, but in all probability shifts back, and persists as the adult anus.

"4. The anterior pair of mesoblastic somites give rise to the swellings of the pre-oral lobes and to the mesoblast of the head.[1]

"There is no need for us to enlarge upon the importance of these facts. Their close bearing upon some

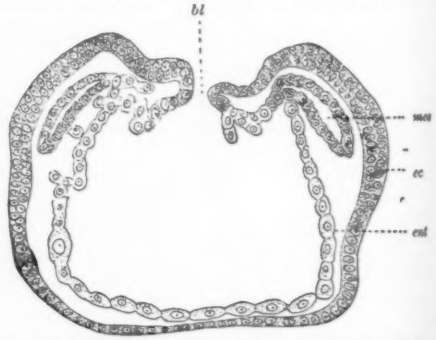


Fig. 7. Section through the open blastopore of the embryo drawn in fig. 6: *bl*, blastopore; *mes*, mesoderm; *ec*, ectoderm; *ent*, entoderm.

of the most important problems of morphology will be apparent to all."

The paper terminates with a few appropriate and telling quotations from Balfour's 'Comparative embryology.' The memoir displays the best qualities

[1] Balfour: *Quart. journ. microsc. sc.*, xix. 1879.

[1] "We have seen nothing in any of our sections which we can identify as of so-called mesenchymatous origin."

of Balfour's work, and can only enhance the respect which all biologists feel for him.

[NOTE. — Since writing this notice, I have learned of the paper since published by Gaffron upon Peripatus (*Schneider's Zoologische beiträge*, i. 33). The original I have not seen, but only a notice in the *Biologisches centralblatt*, iii. 319. From the latter it appears that Gaffron has independently observed many of the facts discovered by Balfour, and in some respects has added to them. The following is the abstract of his description of the heart. "As in the tracheate arthropods, it lies in a special pericardial sinus, completely embedded in a cellular mass, most developed laterally. Its walls are perforated by fissures, corresponding to the body-segments, and which must be sought in the upper half of the tube. Along the dorsal median line runs a round cord, which is held (probably wrongly) to be a nerve. The pericardial sinus and the body-cavity communicate through numerous oval openings in the septum."]

CHARLES SEDGWICK MINOT.

LETTERS TO THE EDITOR.

Prairie warbler in New Hampshire.

Several seasons ago the prairie warbler (Dendroeca discolor Bd.), was found nesting at Northfield in New Hampshire, in June I believe, though I cannot give the exact date. Two of the nests, however, and an egg, are preserved, and place the identity beyond question.

The locality was a high, bush-grown pasture in the vicinity of a town; and the nests were pitched about head-high from the ground, in the crotch of a thorn-bush. The birds made no demonstrations at the approach to their haunts, but retired noiselessly, seeking to screen themselves from view. One nest contained three eggs, a second four. They are substantially the same, finely and firmly wrought, cup-shaped structures, with a well-turned rim. In the latter instance, the external depth is 2½ inches, the internal 1¼; outer diameter 2½, inner 1½. The nest is composed essentially of bark strippings, Andromeda chiefly, fine grass, and blasted vegetable fibre intermingled, and lined with hairs and the reddish filaments of Polytrichum. The exterior is covered with much cobweb silk and some soft compositaceous substance, which serves to compact the whole and secure it in position.

The egg is pointed at one end, dull white, rather finely and sparsely specked with lilac and marble markings, aggregating in a circle about the crown, measures .68×.50 inches, resembling occasional specimens of the chestnut-sided warbler.

So far as I am aware, there is no previous authentic record of this warbler breeding north of Massachusetts in New England. F. H. HERRICK.

Kalmia.

In your issue for Aug. 17, Dr. Abbott doubts if Kalmia grows sufficiently large to be used for making spoons. The abundant thickets of Kalmia latifolia, beautiful but troublesome, are among the clearest recollections of my youth in southern New Hampshire. This shrub is there familiarly known as 'spoonhunt;' and its stems, near the ground, are not infrequently three or four inches in diameter.

CHAS. H. CHANDLER.

Ripon, Wis., Aug. 23, 1883.

Letters in a surface film.

Can any one suggest an explanation of the phenomenon described below?

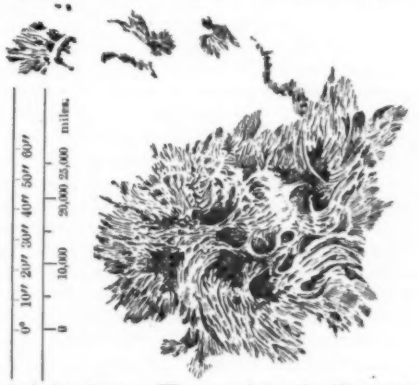
In a box four feet square, and sunk five feet below the surface of the ground, was a water-meter connected with pipes for supplying a factory. Over the face or dial of this meter was a cast-iron cover, on the outside of which the maker's name was inscribed in raised letters. During the spring thaws, the box was half full of surface-water, submerging the top of the meter some eight or ten inches. After a time a greasy film collected on the water, and in this film appeared a counterpart of the raised letters. That it was not a reflection or other optical illusion, was proved by carefully introducing a shovel under these filmy letters, when they were raised and taken outside of the box, being still visible.

In the course of a few hours, fresh letters would appear on the surface. A. P. H.

Boston, Aug. 28, 1883.

An interesting sun-spot.

Owing to a misunderstanding, the scale given with the sketch of a sun-spot, in the letter from S. P. Langley and F. W. Very (SCIENCE, ii. 266), was



printed too large. We reproduce the illustration showing the spot, with a corrected scale. — ED.

A CRITIQUE OF DESIGN ARGUMENTS.

A critique of design arguments. A historical review and free examination of the methods of reasoning in natural theology. By L. E. HICKS, Professor of geology in Denison university, Granville, Ohio. New York, *Charles Scribner's Sons*, 1883. 11 + 417 p. 8°.

THAT men can talk about the most serious problems without passion, is certainly shown by our author, whose candor and excellent aims have already been recognized on all hands. For the rest, we must regard the book with mixed feelings. When we undertook to read it, we did not go forth to see a reed shaken by the wind, nor did we find such; we did not venture to look for a prophet, nor did we find one: but we were prepared for just a

little more definiteness of philosophic thought, for just a little more acquaintance with the history of the subject, and, in general, for just a little more strength. But we must not be too exacting. This is the work of a student of a special science. He comes with suggestions that have been a good while in maturing; he expresses himself in clear language, with great and generally successful effort at fairness; and he shows no small ingenuity. His book will do good both to theological and to scientific students if they read it. And it can do no harm to philosophy. Such discussion is, in fact, so timely that one cannot wish that the book had been kept any longer out of print; but one must wish that the author had begun to study the history of thought a good deal earlier. Achilles at the trench will always be a sublime figure; but the lack of armor is not just that feature in the situation of Achilles which it is safest for other people, at other trenches, to imitate.

The argument from design, says the author, is in fact twofold. In one form it is teleological. Certain events or things are judged to be intended for certain purposes. This argument has less significance for the men of to-day than it had for former generations. The advance of science throws it somewhat into the shade. But the advance of science itself tends to bring into clearer light the other design argument. This is the argument from the *order of nature*. Order, it maintains, implies intelligence, is itself a mark or sign of mind. The more order we discover, the more intelligence is indicated in the world. This does not necessarily mean that we infer intelligence as the *cause* of order; but it means that we regard order, however it may actually be connected with intelligence, as a *mark* of intelligence. This argument needs a name; and Mr. Hicks proposes to call it the eutaxiological argument, to distinguish it from the teleological.

The teleological argument alone is not satisfactory. To prove that any thing implies intelligence as the cause whereby it was adjusted to an end, you must know what the end or purpose of this thing is. And to do this, you must know that there are ends or purposes for things at all; but to assume that you know this is to beg the question. Teleologically, therefore, intelligence as the cause of things cannot be proven; but only particular adjustments, made by an intelligence already known to be the cause of things, can be teleologically discovered. Teleologically you could at best show, that, if there is intelligence in connection with the world as a whole, then this intelligence works for certain special aims. But teleologically it would be impossible, without aid from some other source, to make certain that any mind at all is associated with the world as a whole. It is impossible 'to prove the *existence* of intelligence by means of the definite *direction* given to intelligence,' because the existence of intelligence 'must be *assumed* in order to ascertain its direction.'

On the other hand, maintains our author, the eutaxiological argument escapes the analogous objection. Teleology has to assume the existence of purpose, in order to use it as a proof of intelligence. But eutaxiology has not to assume the existence of order. Order is the first and last word of natural science; and from first to last science continues to deepen the meaning, and to widen the application, of the word 'order.' The difficulty of the eutaxiologist begins not at this point, but later. Are we sure that order is a sign of intelligence? An orderly arrangement of things is a mark of intelligence in many cases. "Suppose we find smooth stones or shells on the beach, arranged at regular intervals in a straight line, or in three straight lines to form a triangle: we should say that an intelligent being had done this." To be sure, in this case we should suppose that some man had done it; but that would not affect the matter, for, "if we saw such figures upon the moon or upon any of the planets, we should at once conclude that they were inhabited by intelligent beings." Thus in these cases, reasons Mr. Hicks, order is inductively connected by us with intelligence. "We see intelligence producing orderly results; and we project the inference thence derived over those cases of orderly phenomena of which we do not know the cause." But what is done in special cases of order observed in forms or in groupings of objects, ought fairly to be done in regard to the whole of nature; and that especially because every case of orderly connection that we find, and that suggests intelligence, is found not alone, but itself in connection with other cases, so that we could not finally stop with our examination of one case of order before we should know its connections with the whole of the rest of the universe. The more, then, we know of nature, the more orderly and connected does it seem, and the more reason we have to apply our induction to the world as a whole.

All this, of course, implies no definite view about the way in which intelligence is connected with the order of the universe. Whether it be that arbitrary collocations of matter are

the immediate sources of the order, or whether the order follows from the fundamental properties of matter, the result is the same. And for a like reason eutaxiology has nothing to say of divine attributes over and above intelligence. Eutaxiology does not even by itself prove the existence of God. It simply proves that intelligence exists in the universe. It leaves to other proofs the discussion of other divine attributes. Eutaxiology having proved intelligence, teleology can then be used to prove that this intelligence is somehow associated with will and power, and works (through evolution or otherwise) for definite aims; and other proofs may be used for other purposes. In conclusion, why may not the various theistic arguments agree to divide labor, and combine the outcome, so that each one shall undertake to prove just that divine attribute to whose defence it is especially fitted? Thus confusion might be avoided, and the cause of natural theology advanced. Mr. Hicks even goes so far as to suggest, in a very generous outburst (p. 389), that possibly that despised creature, the ontological proof, might find some kind of mission in the midst of his desired association of theistic arguments. The ontological proof, having very long been able to say, —

> "I lie so composedly now in my bed,
> That any beholder might fancy me dead,"—

must regard the kindness of Mr. Hicks with very mixed emotions. He thinks that it might be 'just the thing to supplement' the others. But during its natural life the ontological proof used to think that the others might possibly be of use to supplement itself.

Such, then, is our author's own line of argument. Between the introduction and the final exposition of this argument, he inserts a discussion of the history of design arguments. This is a mere collection of notes, with more or less ingenious reflections that suggested themselves to the mind of the collector here and there in the course of his work. The 'Natural theology of the Greeks and Romans' is treated in some thirty pages, which are devoted to Socrates, Cicero, and Galen. How, one may ask, would it look for one to head a chapter with the title 'The astronomy of modern times,' and then to treat the subject by briefly expounding some statements of Galileo, Lord Brougham, and Dr. Whewell? Thirty pages might well be the limit allowed by the plan of our author; but such a space is not too limited for a really connected historical sketch, with some attention to the perspective in which every man's thought ought to be viewed. The author's account of Spinoza is similarly imperfect, because no effort has been made to see what the man, with his odd, crabbed method, really had in mind. We are told, what we all knew before, that Spinoza's method is unsuccessful; but, for the rest, we learn more in this chapter about Mr. Lewes than about Spinoza. 'Reimarus, Kant, Hume, and Reid' are somewhat embarrassed to find themselves side by side in one chapter; and poor Kant especially is made to speak as he did in 1763, instead of being allowed to present himself as he does in the 'Critique of pure reason,' nearly twenty years later. Although this error is in just this discussion not so serious as the corresponding error would be in expounding other parts of Kant's doctrine, yet the method is unhistorical; and the result is, that, in summing up, Mr. Hicks hopelessly confuses Kant's pre-critical and critical periods. In short, our author shows himself in general no historian of thought. Throughout the whole sketch, there is a lack of a sense of the development of thought. Each man's notions stand beside his neighbor's, as if the philosophers were all speakers in a debating-club. And Mr. Hicks, as intelligent listener, adds his applause and his comments in brackets, and is not afraid to express himself with even boyish freedom of speech. But he is always good-humored, and his criticisms often hit the mark very well. Yet it is to be hoped that nobody will undertake to judge the history of natural theology on the basis of this account.

Now as to the result. What shall we say of eutaxiology? We have no hesitation in declaring the argument, as our author presents it, an altogether defective one. For, as he presents the eutaxiological argument, it is an inductive argument, and solely inductive. If we saw a triangular arrangement of objects on the moon, we should conclude that some intelligence had done this. We should extend the known association of intelligence and order, as we find it about us, to cases of order more remote from our direct observation. We should conclude that order is a sign of intelligence, even where we have no other evidence of the presence of intelligence. So reasons Mr. Hicks. But is this sound? And, first, is the author's suggestion about the supposed geometrical figure seen on some planet a correct one? Should we, if we saw such a figure on some planet, at once conclude that intelligence had caused it, or was in any way associated with it? Surely not everybody would feel the force of such an induction.

Most scientific astronomers, observing such a regular figure for the first time, would at once look for some ordinary physical explanation of its presence, even as they now try to explain the shapes of the planets; and, failing to find such an explanation, they would be content to call the triangle a mystery. Only some man whose position as a public lecturer on astronomy demanded that he should have a new sensation ready for each new lecture-season would be apt to insist on the existence of some set of geometrically disposed planetary giants. More sober people would be content with an *ignoramus*. But how much less satisfactory becomes such an induction when applied to the whole of nature! At best would not such an argument be like the inductive reasoning of a man, who, having already learned the modern doctrine of the relation of the colors of flowers to the habits of insects, should for the first time, and without any previous knowledge of marine zoölogy, find a colored shell by the sea-shore, and who should then at once expect to find some race of insects in some analogous relation to the inhabitant of this shell? Or, again, if one extended even to the rainbow, or to the sunset, an explanation derived from the case of colored flowers, and their relations to insects, would not the argument possibly be no more absurd than the induction upon which Mr. Hicks lays so much stress? Men and beavers and other creatures make orderly groupings of things. Hence order implies intelligence, and that wherever we find order. Is this argument any better than the old teleology? Mr. Hicks is deceived, it would seem, by the vast wealth of facts to which his argument appeals. He neglects the difficulty of bringing such various facts within the control of an induction that has for its narrow basis such intelligent activity as we see about us among men and animals. As induction, pure and simple, eutaxiology seems to us simply worthless.

But is the order argument in any form therefore worthless? Certainly not. Mr. Hicks does fine service in bringing before the public, just at this moment, a thought that is by no means new, and that is profoundly suggestive. 'What does the order in the world imply?' This is a great question, not of inductive science, which is concerned solely with discovering the actual order itself, but of general philosophy. And Mr. Hicks is, we doubt not at all, quite right in saying that order implies intelligence. But how, and what intelligence? Such questions he leaves wholly unanswered. The critical philosophy of Kant would, strictly speaking, affirm that order in the world implies only the intelligence of the thinking subject to whom the world appears. The world is orderly, because only as orderly could it become known to an intelligent being. Not the world in itself, but the world for thinking beings, is to be viewed as orderly. This view would make short work of our author's 'induction,' but it would not satisfy him. He would then need to know and build beyond Kant. In short, Mr. Hicks has very ingeniously set his reader down at the beginning of a great philosophic problem. It would argue a lack of intelligence in the reader if he did not seek to bring his thoughts into a better order than that in which Mr. Hicks will have left them; and the author's service lies in making it impossible for an inquiring mind to rest content with what is here offered to him. This, however, at least, he has very well suggested, though he has not proved his suggestion: viz., that the postulate of natural science is the rationality of the world. Whether we find order, or only seek it in nature, we are always *a priori* sure that the world is actually full of connections that admit of expression in rational terms, of explanation to an intelligent mind. And so we assume a fundamental likeness of nature and intelligence that suggests to us very strongly some kind of real unity or identity of nature and intelligence. But whether this suggestion has any ground, whether this identity of nature and mind is to be accepted at all, or is to be accepted in Kant's sense only, or in Berkeley's sense, or in Hegel's sense, or in some other sense, this is a matter for philosophy to discuss. We thank Mr. Hicks for having shown afresh the necessity for such discussion. His eutaxiology is not so original as he thinks; but his offering on the altar of philosophy deserves the reward due to every gift that a special student of natural science finds time to offer in the true spirit of calm investigation.

MAYNARD'S MANUAL OF TAXIDERMY.

Manual of taxidermy; a complete guide in collecting and preserving birds and mammals. By C. J. Maynard. Boston, *S. E. Cassino & Co.*, 1883. 16 + 111 p., illustr. 12°.

A really *complete* guide in collecting and preserving the objects named in the title of this work, which can safely be relied upon for information under all circumstances and in all climates, has long been sorely needed by the host of amateurs, taxidermists, travellers, and even professional naturalists interested in verte-

brate zoölogy. Notwithstanding the presence of the neat little volume before us, and its promising title, a complete guide is still as much a desideratum as ever. Like all other books which have appeared in English on this subject, this volume is small and thin, and, we are compelled to add, wretchedly illustrated. Of the one hundred and one pages of subject-matter, sixteen are frittered away in an effort to inform the reader where birds of the various families from Turdidae to Alcidae are to be found. How much better to have devoted this space to adequate instructions for mounting dried skins, which important branch of the subject is summarily disposed of on a single page, instead of to such cheap information as that 'the chimney-swift inhabits chimneys,' that kingfishers are found 'in the vicinity of streams,' and the like. With the exception of the above, all the information and advice contained in the chapter on collecting is valuable, and bears the stamp which experience places upon its work.

The chapters on 'skinning birds' and 'making skins' would be very satisfactory but for one thing. While the author strongly condemns dry arsenic as a dangerous poison, and says not a word about arsenical soap, the only preservative he recommends as fit for use is one compounded only by himself. After extolling its virtues to the extent of two pages, but carefully withholding all information as to its composition, he coolly informs the reader that its price is 'twenty-five cents per single pound.' We are told that tannic acid, alum, salt, or black pepper (!) may be used to temporarily preserve skins until the other can be procured. The 'dermal preservative,' which, strange to say, is not a poison, is recommended, or rather exclusively directed, in no fewer than fourteen places throughout the work, for mammals, birds, reptiles, and fishes, as a non-poisonous astringent, absorbent, deodorizer, and insecticide; and, if the reader is at all credulous, he will be led to exclaim, There is but one preservative, and C. J. Maynard is its maker! If this little book is honestly intended to meet the wants of amateur collectors wherever it may find them, and not to increase the sale of a nostrum of doubtful value, nor to advertise the author's business, the author has taken a queer way to show it. It will not be surprising if his readers resent such unfair treatment.

While there is much that is practical, valuable, and new in the chapter on mounting birds, and in those detailing the treatment of mammals, reptiles, and fishes, they are all deplorably incomplete; and we vainly regret that the author did not go as deeply into the subject, and with as good diagrams and illustrations, as he might have done. The information given is valuable as far as it goes; but there are only one-quarter as many facts stated, and directions given, as the unskilled operator needs to know.

As an example of the doubtful value of such highly condensed instructions, we may take those for skinning small mammals. The author says, " . . . peel down on either side [of the body] until the knee-bones are exposed, then cut the joint, and draw out the leg, *at least as far as the heel.*" Not a word is said about skinning the foot, and removing the flesh under the metacarpal and metatarsal bones: hence we suppose it is left to decompose, which it will generally do right speedily, and at the expense of the hair and epidermis above. We should like to see the author remove and prepare the skin of any monkey according to his own directions.

We are honestly sorry we cannot freely recommend this manual — nor any other in our language, for that matter — as being well calculated to meet the wants of those for whom it is intended. An epitome of the subject is no longer wanted, but a handbook which shall be really complete is needed very much.

ELEMENTARY TREATISE ON THE MICROSCOPE.

Traité élémentaire du microscope. Par EUGÈNE TRUTAT, Conservateur du musée d'histoire naturelle de Toulouse. Paris, *Gauthier-Villars*, 1883. 322 p., 165 ill.

FEW are aware of the magnitude to which microscopical work has grown. The modern methods of research in the physical and biological sciences have involved more and more an appeal to the microscope. As a result of this growth, we find whole volumes devoted to a description of the microscope and its application to the various departments of study.

Microscopy has been taught in our schools only a very few years. This is partly due to the fact that formerly the instruments were both expensive and imperfect. There was also an almost total lack of literature upon the subject. At the present time, however, there are plenty of good works on microscopical technology, and the microscope as applied to the study of medicine in all its branches, including biological research.

In a work like this before us, it is necessary to present a large amount of material of such an elementary character that it is of value

only to the novice. It is decidedly a French work, written by a true Frenchman. Neither an instrument nor an accessory is mentioned, unless either invented or manufactured by a Frenchman. The stands of Verick are given great prominence, as are also those of Hartnack. When we consider how beautiful and useful are the instruments of our own country, to say nothing of the fine productions of English houses, we are forced to call the work 'an elementary treatise on the French microscope.' For convenience, elegance of design, and varied adaptability, the French microscope will not compare with those of our own country, while we far excel in the superior quality of our objectives.

The microscopist will be much interested in reading the chapter on the projection microscope. Electricity will soon furnish us with proper illumination.

More information is given under the head of mineralogical research than in any work brought to our notice. Among the accessories mentioned is the camera lucida of Oberhauser. It is a form little used in America, and yet it is one of the most convenient and perfect of its kind.

The new pattern of Malassez's *Compte-globules*, by Verick, is minutely described. The results obtained by this instrument promise to be very accurate: we have practically tested its merits, and can give testimony to its precision. The method for photographing from the microscope is not so simple as that employed here by the use of dry plates; and, if the frontispiece be taken as a sample, it is not more satisfactory. The author shows perfect familiarity with the instruments and accessories, together with their applications as made and used in his own country. C. H. Stowell.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PROCEEDINGS OF SECTION E. — GEOLOGY AND GEOGRAPHY.

Reports of committees on geological subjects.

To the call for a report of the *Committee to memorialize the legislature of New York for a new survey of Niagara Falls*, Prof. James Hall responded, that several surveys had been made, or were in progress, in connection with legislation by the State of New York for preserving the scenery. These would supersede the need of any work of the kind by the association. The committee was discharged.

To the call for a report of the *Committee on state geological surveys*, Prof. N. H. Winchell responded, that the committee had never been called together, and there was no probability of its action. The committee was discharged.

To the call for a report of the *Committee on the international congress of geologists*, Dr. T. Sterry Hunt (by request of the chairman, Professor Hall) responded as follows: —

The committee held a meeting in the month of November last. Two important questions came up, — of geological nomenclature, and topography. It was suggested by Professor Hall, that the only action which could be taken in support of the system of uniform mapping and colors, and signs and symbols, would be to prepare maps of the United States as a whole, and perhaps also maps of portions of the United States, and to color them by different systems; the system adopted being that of Major Powell of the U. S. geological survey, and one or two others. Major Powell has been good enough to say that he would endeavor to prepare such maps, and aid in every way the carrying-out of the scheme. I have no doubt that the matter will be so well managed that the whole question of geological topography will be settled.

As to the question of geological nomenclature, we had much difficulty in getting reports of the previous meetings; and we have named several persons, some of whom have already handed in, or have in process of preparation, their abstracts of geological nomenclature; and I have every reason to hope that in the course of a few weeks we shall have the whole of that matter in shape to transmit to the Berlin congress a full and proper representation of the views of American geologists with regard to our geological nomenclature. There is one thing very much to be regretted, — the possibility that the meeting of the American association and the British association will come in collision with the meeting of the Berlin congress. Nothing definite has been arranged, so far as I can learn by letters. I have met with no response, but I was told that the time of the Berlin congress had not been fixed. In the committee which was held to consider arrangements for the meeting of the British association, it was suggested that we put ourselves in communication with the local authorities of the Berlin congress, and endeavor to get them to fix the time of their meeting so late in September as will allow members of the American and British associations to leave this continent after the meeting of our associations so as to be present at the Berlin congress.

The committee was continued.

The *Committee to confer with the United-States geologist in regard to co-operation between government and state geological surveys* was called on for a report. Prof. James Hall of Albany responded in-

formally: The condition of the state survey is likely to be materially influenced by the law of the general government extending the U. S. geological survey over the states. Proper deference to the head of the U. S. survey required that some action should be taken by which we could confer with Major Powell, to understand our relations to the survey. To prevent any jealousy or uncertainty with regard to what might be the relation of the state survey and the general survey, I suggested the appointment of this committee. I had no intention myself of taking any active part in the matter; and I think there are gentlemen on the committee, much younger than myself, who will do all the work. I believe several members of the committee have had very pleasant interviews with Major Powell, as I have myself, since these meetings commenced; but I had forgotten that I was to make a report. I think it is desirable that there should be very frank intercourse between the gentlemen who are conducting the state surveys and the head of the general government survey, so that we may know what is to be the result of their various surveys which are so very important to geological science. Workers at a distance from each other cannot, without some means of inter-communication, — which, I think, may be established with the head of the general survey, — bring the results of their labors to a fair comparison with those which are done a thousand miles away.

Major Powell expressed the hope that the committee would be continued. Several members of the committee had conferred with him with reference to the surveys, but they had not conferred as a committee. Practical relations have been established between the general survey of the United States and several of the state surveys. He thought it was probable that such arrangements could be established as would make it satisfactory to all.

The committee was continued.

PAPERS READ BEFORE SECTION E.

(PAPERS ON GLACIAL PHENOMENA.)

The life history of the Niagara river.

BY JULIUS POHLMAN OF BUFFALO, N.Y.

A SERIES of observations whose points were given in detail had convinced the author that the formation of the gorge of Niagara had been a matter of tens of thousands, rather than of hundreds of thousands, of years. The beginning of the history might be stated as in the pre-glacial epoch. A lake then occupied the valley of the Tonawanda; its outlet was the line of the ancient Niagara River from the falls to the whirlpool; thence, by way of the St. Davids valley, into the Ontario valley. All these valleys were closed during the glacial period. The subsidence of Lakes Erie and Ontario was that of one body or region, until they were separated by the Lewiston escarpment; after that the drainage of Lake Erie found its path through drift deposits and old existing valleys to Lake Ontario. The latter lake subsided slowly, and no waterfall was formed at its entrance. The river excavated its gorge to the whirlpool, not by means of a retreating fall, but as a rapid in an old shallow valley. At the third pool, this path met the ancient river-valley: it was along that valley only, that the falls receded to their present site. The retreat of the fall was not the means of excavation, for at least seven miles usually ascribed to it; the portion which would offer the most resistance, between the falls and the whirlpool, being already excavated.

From that point to Lewiston, the progress was very rapid in cutting the gorge; a shallow valley had partly removed the hard limestone, and the softer underlying shale rock was a barrier much more easily penetrated. We have no exact data of the retrocession of the falls within periods of modern observation. A comparison of Professor Hall's map of the falls in 1841, and that of the United-States lake survey in 1875, shows wide discrepancies. After all reasonable allowance for inaccuracies, we must admit that parts of the Horse-shoe fall have receded in thirty-four years at least one hundred feet, and on the American side the recession is from twenty to forty feet. These facts all tend toward a shortening of the history of the present river.

In the discussion that followed, Professor Hall expressed a doubt as to the dependence that could be placed on differences between surveys made by different persons, using differing methods. That there had been retrocession within the period of our observation, he did not doubt; but it could scarcely be so rapid as was indicated by the estimates of Dr. Pohlman. Other speakers discussed the paper, which was of special interest, because it fired the first gun of the glacialists in the geological section, and it roused their opponents.

Glacial cañons.

BY W. J. McGEE OF SALT LAKE CITY, UTAH.

THIS paper was read, in the absence of its author, by Mr. Warren Upham. It considered the action of a glacier as being, to a certain extent, capable of representation by mathematical formulae. It was admitted, however, that some of the quantities in the equations must remain very indefinite. The paper was almost wholly theoretical, and arrived at the following conclusions: The temporary occupancy of a typical water-cut cañon by glacier-ice will, 1°. increase its width; 2°. change the V to a U cross profile; 3°. cut off the terminal portions of tributary cañons, and thus relatively elevate their embouchures; 4°. intensify certain irregularities of gradient in the cañon bottom; 5°. excavate rock basins; 6°. develop cirques; and, in general, transform each cañon into an equally typical glacial cañon. It follows that these features do not necessarily imply extensive glacial excavation, or indicate that glaciers are superlatively energetic engines of erosion.

Owing to the custom of abstaining from discussion on a paper in the absence of its author, the dissentient opinion of many who were present was not

fully elicited. The general expression was to the effect, that the theory had been framed without sufficient observation of the facts, and that, if the author had taken the trouble to see and examine various cañons, he would have come to a widely different set of conclusions.

The ancient glaciation of North America: its extent, character, and teachings.

BY J. S. NEWBERRY OF NEW YORK.

While the glacial area on our continent has not been fully explored, there is abundant proof for the following propositions: 1°. Glaciers covered most of the elevated portions of the mountain belts in the far west as far south as the 36th parallel, and in the eastern half of the continent to the 40th parallel of latitude. 2°. The ancient glaciers, which occupied the area above described, were not produced by local causes, but were evidences of a general climatic condition. 3°. They could not have been the effect of a warm climate and an abundant precipitation of moisture, but were results of a general depression of temperature.

The traces of glaciation are similar in kind, and apparently in date, over the whole area: they are therefore effects of general, not of local, causes. East of the Mississippi, the evidence is even more widespread and impressive than in the far west. The area bearing marks of ice action, and strewn with drift, extends from New England westward, parallel with the Canadian highlands, in a belt five hundred miles wide and over two thousand miles long. Its northern extension has not been traced beyond Winnipeg; but there are reasons for believing that it reached to the Arctic ocean, and that the great lakes are pre-glacial river-valleys, scooped out and modified by ice. Fully half the continent north of the 36th parallel was glaciated. So far as we now know, the glaciation was synchronous.

The iceberg theory was opposed by Dr. Newberry, on the following grounds: It postulated a water-line with irregularities of level that are irreconcilable. The direction of the scratches, and the lines of deviation of the bowlders, require that the northern portion of the continent should have been all submerged, leaving no land for the origin and starting-point of icebergs. If the icebergs could have been formed and floated, an incomprehensible tangle of ocean-currents would be required to account for their movements. The evidence of sea-covering, in the form of marine shells, is totally absent from the great drift area of the interior, while they are found abundantly in the Champlain and bowlder clays of the coast. Finally, the inscription left by the eroding agency is characteristic and *sui generis*.

The record of the ice period on our continent is far more extensive and impressive than it has been represented. The phenomena were due to an extraneous and cosmical cause, not to any thing local or even telluric. The question here passes from the geologist, and must be addressed to the astronomer. Professor Newberry briefly recapitulated some of the theories which have been suggested by Croll, Newcomb, and others, to account for the glacial epoch.

Result of explorations of the glacial boundary between New Jersey and Illinois.

BY G. F. WRIGHT OF OBERLIN, OHIO.

After citing reasons for desiring a careful *résumé* of the subject, — the observations being scattered in the works of different explorers, — the author proceeded to name those who had determined, for different regions, the southern boundary of the glacial area. Starting at the eastern coast, President Edward Hitchcock was the first to intimate that the backbone of Cape Cod was a part of the terminal moraine if the theory of Professor Agassiz were true. Clarence King made a similar assertion as to accumulations near Wood's Holl and on the Elizabeth islands. Professor Charles H. Hitchcock declared that the backbone of Long Island was the foot of a terminal moraine. Warren Upham went over this field, from the end of Cape Cod to Brooklyn, to verify the hypothesis. Professors Cook and Smock traced the moraine across the state of New Jersey. Professor Lesley commissioned Professor Carvill Lewis and the author of the paper to continue the exploration across Pennsylvania. In Ohio, Professor Newberry has approximately outlined the boundary; but in Ohio, Indiana, and Illinois, the survey was carried on by a number of different persons before the most distinctive glacial features were fully understood.

The chief indications on which reliance can be placed to determine glacial action are striated rocks, striated stones, bowlders, and till. Rocks near the margin are often so deeply embedded in till, that their markings are not apparent. The softer rocks do not always retain their striae: this has often been the case in Ohio. In certain situations, stones might be striated by a landslide, or the grounding of an iceberg; but the area over which striated stones are found is too vast for such explanation of their presence. The bowlders are of granite and metamorphosed rocks from northern Canada and the shores of Lake Superior: their presence is relied upon only when they are on such high lines as to preclude the likelihood of their having been transported by the agency of rivers. Till is spread over the whole area: it is defined as an unstratified deposit, containing striated stones of various sizes, — fragments of rock foreign to the locality. Its composition varies, through mixture with underlying material. It covers and gives fertility to northern Ohio, Indiana, and Illinois. Till has been characterized by Professor Newberry as the grist of the glacier.

Briefly told, the boundary-line of the glaciated area, so far as now accurately known, is as follows: Beginning on the island of Nantucket, it runs through Martha's Vineyard, No Man's Land, Long Island from east to west, across Staten Island, entering New Jersey at North Amboy, and after bending northward and making a right angle near Dover, crosses the Delaware at Belvidere. Thence it runs north-westerly through Northampton, Monroe, Luzerne, Columbia, Lycom-

ing, Tioga, and Potter counties in Pennsylvania, and Cattaraugus county, New York, reaching its most northerly part about five miles north of Salamanca. From here it runs through Warren, Venango, Butler, Lawrence, and Beaver counties, to the Ohio line, crossing Beaver creek at Chaintown about fifteen miles above the Ohio river.

The boundary enters Ohio in the northern part of Columbiana county, and proceeds nearly west to the middle of Stark; then turns more to the south, touching the corner of Tuscarawas, and dividing Holmes into two nearly north-and-south sections. Near the north-east corner of Knox, the line makes a right angle, and runs south through Knox, Licking, the north-west corner of Perry, Fairfield, Ross, Highland, Adams, and Brown counties. Then it follows the line of the Ohio river across Clermont, and enters Kentucky near the boundary between Pendleton and Campbell counties, and, after crossing the northern part of Kenton and Boone counties, recrosses the Ohio, entering Indiana a little below Aurora.

In Indiana the line still continues to bear in a southerly direction through Ohio and Jefferson counties, grazing the edge of Kentucky again opposite Madison, and reaching its southernmost point near Charleston in Clarke county, Ind. From here it bears again to the north, through Scott and Jackson counties, to the line between Bartholomew and Brown, and follows this to the north-east corner of Brown. There again it turns to the south-west, touching the north-east corner of Monroe, where it again bears north for ten miles, to near Martinsville in Morgan county. Here again the line turns west and south, passing diagonally through Owen and Green counties, and in Knox as far as Harrison township, ten miles south-east of Vincennes. Beyond this point, the author did not propose at present to trace the line.

The signs of glaciation cease where there is no barrier to account for their cessation, and where no barrier ever could have existed such as must be supposed if the so-called glacial phenomena are the product of floating ice. Of the correctness of this inference, the different elevations at which the signs of glacial action cease are sufficient proof. For instance, the line is near sea-level in New Jersey; in Pennsylvania it rises over Mount Kittatinny to a height of 1,200 feet, then descends 800 feet into a valley, and, again rising, reaches the summits of mountains 2,000 feet above sea-level. Crossing the valley of the Susquehanna at an elevation of only 500 feet, the line mounts the Alleghanies diagonally, and runs over them at a height of 2,500 feet.

The paper proceeds to describe certain marked features of glaciated areas. South of New England, the terminal line is characterized by a series of glacial hills, 100 to 300 feet high. These are also observable in New Jersey, near Plainfield and Menlo Park.

Among the most interesting results of the author's survey in Ohio, was the demonstration of the existence of an ice-dam across the river at Cincinnati. The line bounding two glacial accumulations crosses the Ohio river into Kentucky, near the boundary between Campbell and Pendleton counties, about twenty-five miles above Cincinnati, and recrosses it near Aurora, Ind., about twenty-five miles below Cincinnati, thus filling the channel for about fifty miles of its course. The Ohio, it should be said, occupies, throughout nearly its whole extent, a narrow valley of erosion, not often more than a mile wide, and from 300 to 500 feet deep. Emptying into the main channel there are subordinate channels all along, of smaller dimensions, but of nearly equal depth. The proofs that the ice bodily crossed the river at the point indicated are, that till and granitic bowlders are found in the Kentucky hills south of the river to a certain distance, and not beyond it.

To the question, Why is the boundary of the glacial area so crooked? the author replied at some length; assigning as a principal cause, aside from differences of level, the probability that unequal amounts of snow fell over different regions of the north. The effect of such differences of accumulating snowfall, in determining the extension of the glacial outline, is illustrated by supposing that two loads of sand are placed in one pile, and one load in an adjoining pile; when the sand will flow downward to unequal distances upon a level.

A little reflection will show that the glacial theory will not make extravagant suppositions as to the amount of ice required. The ice was indeed 600 feet deep over New England, and, very likely, of an equal depth over the area to the west; but it is not necessary to suppose a great increase of this depth to the north. All that is necessary is to suppose great accumulations of ice to the north of the granitic hills of Canada, starting a movement past them to the south. This movement may have been kept up toward the margin by fresh accumulations of snow upon the spreading glacier. An accumulation of snow over the glacier in any part of it would spend its effective force in giving impetus to the movement of the front along the lines of least resistance.

The discussion which followed the reading of this paper took a wide range, as the paper itself contained many points of interest. The opponents of the glacial theory, or of the younger theories which have sprung from its loins, based their criticisms chiefly upon doubts of the evidences of glaciation. The questions raised, as to the distinctive characteristics of glacial and subaqueous deposits, gave tone to the paper of the next speaker, which was delivered orally, and was, at least in part, extemporaneous.

The terminal moraine west of Ohio.

BY T. C. CHAMBERLIN OF BELOIT, WIS.

THIS paper was introduced by a statement of the author's views on some points that had been alluded to in the discussion of Professor Wright's paper. Dr. Chamberlin had himself observed the features of the drift-bearing area west of the Rocky Mountains. Certain of the drift clays are unquestionably glacial: others have quite as certainly had a wholly different origin. He specified with great particularity the

means for discrimination between the clays, but admitted that there were instances where the different types seem to blend insensibly into each other.

West of the Scioto valley, the border of the drift-bearing area is not marked by what is regarded as a moraine. There is, however, an extension of what Professor Wright has characterized as the 'glacial fringe,' consisting of bowlders. In Dakota county, Minn., this fringe is very wide. At Crystal lake there is a well-marked moraine, and possibly there is another a little to the westward. Farther to the west, there is no accumulated morainic drift. West of the Missouri, there is no evidence of glacial ploughing.

A line of drift-hills known as the Potash Kettle range, in eastern Wisconsin, had been regarded as an old beach-line. Dr. Chamberlin has ascertained that the range is a glacial moraine. He described it as an interlobate moraine, formed jointly by glacial lobes occupying the valleys of Lake Michigan and Green Bay, respectively. This was correlated with moraines to the westward in Wisconsin.

Furthermore, there was a system of moraines, — a belt or group, including the glacier lobes at Lake Michigan, in the Chippewa valley, at the western extension of Lake Superior, and in the valley of the Minnesota River, and Red River of the North. These moraines were more pronounced, with a few exceptions, than those on the outer edge. Investigations were being carried eastward with a view of showing their correlation with other moraines in that direction. The hypothesis of their exact correlation, of course, would imply that they were contemporaneous; but there are doubts upon that point.

The author claimed, that there were evidences that the lake-basins were caused in part by depressions during the ice age, caused by the exceptional accumulation of ice in the basins. He deprecated the notion that subsidence must always take as long a period as elevation, or that the reverse is true. He applied this to the case which he alleged of the depression during the presence of ice in the lake-basins, and the elevation since.

In discussing this paper, Professor Lesley said it was time to cry halt as to this theory of depression by weight of ice. It was made to do duty for a great variety of emergencies. In point of fact, ice was much lighter, very much lighter, than any rock. Professor Lesley pointed out instances where this theory had been advanced to account for depressions which now contained a greater weight than the ice could have made with any reasonable hypothesis of its thickness.

Professor Chamberlin explained the theory further, and claimed that in instances which he cited the depression was greatest at the weakest part of the strata.

Prof. E. S. Morse referred to some English experiments to determine the question whether the moon's attraction deformed the earth's outline. It was found (according to newspaper report), that the weight of the incoming tide deformed the surface to such an extent that the effect of the moon's attraction could not be separately calculated. Major Powell called attention to the theory, that, if the earth were divided into conical sections radiating from its centre, there would be found an equal pressure in each. Every sediment, every erosion of the surface, must be balanced by corresponding depression or rise elsewhere. Finally, the case of Lake Saltonstall was cited by Mr. Hovey. It is evidently situated in a valley that was ploughed out by the foot of a glacier; certainly not in a hollow caused by pressure. Professor Cox clung to another theory entirely, as to the great lakes. He believed them to be prolongations of a sea-coast which had at one time extended to them through the valley of the St. Lawrence.

The Minnesota valley in the ice age.

BY WARREN UPHAM OF MINNEAPOLIS, MINN.

THE paper was based upon the author's observations for three years as assistant on the geological and natural-history survey of Minnesota, under the direction of Professor Winchell. To the question: During what ages was the glacial, rock-walled channel of the valley of the Minnesota River formed? — the paper offered an answer. Deposits of cretaceous clay were found in water-worn hollows at several enumerated localities; and, in other places, cretaceous sandstone and shale occasionally containing lignite. It thus appears, that, before the cretaceous age, a deep channel had been cut by some river in the lower magnesian sandstone, and the Potsdam formation. The slopes, the drainage, perhaps even the channel, of that river, were not widely different from those of the present; but that channel was probably eroded during the later paleozoic and earlier mesozoic ages, before the cretaceous subsidence.

In the first epoch of glaciation, when the ice covered its greatest area, a thick drift-sheet, mostly unmodified, probably covered all this region, including the preglacial valley, with an unbroken, though undulating, expanse of till. During the ensuing interglacial epoch, the drainage cut a channel, whose position was largely determined by the slopes of the erosion which had preceded the glacial epoch. The preglacial, and also the interglacial river, lay far below the present stream. The till of the later epoch blocked the course of the river only in part of its extent, and the obstacle was soon channelled anew.

During the recession of the last ice-sheet, the valley was filled with modified drift. After the ice was melted in the Minnesota basin, this avenue of drainage was, for a long period, the outlet of Lake Agassiz. The volume of water that it carried was very large, being supplied by the melting ice-fields of Northwestern Minnesota, and from the region of Lake Winnipeg and the Saskatchewan. While streams poured into this river from the melting ice-sheet, its modified drift continually increased in depth; but, when the great glacier had sufficiently retreated, the water from Lake Agassiz not only ceased to contain drift, but became a powerful eroding agent. The deposited drift was mostly swept away, and the channel

was again excavated, perhaps to a greater depth than the present river, possibly to the bottom of the gravel and sand, at a point in the valley which is 150 feet below the river there, and 135 feet below low water in the Mississippi at St. Paul.

When the ice-barrier which had made Lake Agassiz disappeared, that lake was drained northward toward Hudson bay. Thenceforward, the rivers of the Minnesota and Mississippi valleys carried only a fraction of the former volume of water from this source. They have since become extensively filled with alluvial gravel, sand, clay, and silt, brought in by the tributaries of those rivers. The changes produced by this post-glacial sedimentation have been ably discussed by Gen. G. K. Warren, and were briefly summarized in the paper of Mr. Upham.

Lake Superior seems to have been held by an ice-barrier at a level of about 500 feet from its present height. The locality of its overflow was stated, and various results detailed. Lake Michigan, until the ice-sheet receded from its northern border, discharged southward by the Illinois river, which, like the former outlet of Lake Superior, was eventually obstructed by alluvium, so that now it has a very slight current for two hundred miles.

The paper closed with a proposition to call the ancient river of the glacial age, the river Warren, in honor of Gen. G. K. Warren.

The discussion which followed was, in part, a conflict between the glacialists and their opponents, and, in part, a debate upon the general question of naming geological features after distinguished investigators.

Changes in the currents of the ice of the last glacial epoch in eastern Minnesota.

BY WARREN UPHAM OF MINNEAPOLIS, MINN.

WITHOUT a map, or a thorough familiarity with the region referred to, this paper would not convey very definite ideas. Through some inadvertency the map intended to be used was not on hand when the paper was read. The author's observations had led him to conclusions of a very definite character. He conceived, that, when the ice of the last glacial epoch attained its maximum extent, there were two ice-currents. One moved south-westerly from Lake Superior, across the north-east part of Minnesota, spreading a reddish till with bowlders and pebbles, and limited by a line from Lake St. Croix south-west across the Mississippi, and thence bending north-west by Lake Minnetonka, and through Wright and Stearns counties. The other portion of the ice-sheet was pushed from the region of Lake Winnipeg, south and south-east. The two met along a line from Stearns county, south-east by Lake Minnetonka to Crystal Lake, Dakota county. Afterward, when the ice had partly melted and retreated, a second and inner terminal moraine was formed. Owing to climatic changes (the rationale of which was carefully and very explicitly set forth in the paper), the current from the north-west pushed back that from the east, and covered the reddish till, already deposited, with a blue till from the west and north-west, also abundant in its peculiar bowlders and other evidences of its source.

The kame rivers of Maine.

BY G. H. STONE OF COLORADO SPRINGS, COL.

IN the absence of its author, this paper was read by Mr. Upham. After defining and describing the characteristics of kames, and stating that they are very numerous in Maine, where he had observed them, the author proceeded to discuss a single question in relation to these geological features. Most glacialists are agreed that the kame gravels of the drift region were chiefly deposited by glacial streams. The question is, whether these streams were sub-glacial or super-glacial. In exploration during the past five years, the author had found evidences of both kinds of streams; but he nowhere found stratified or even water-classified material enclosed in this formation, except within a few miles of the coast.

The essayist sought to answer the question by considering the processes of melting which take place in a glacier. Strict analogy with existing glaciers — even with those of Greenland — should not be supposed. In modern glaciers, nearly all the water of their lower extremities is sub-glacial. The ice is so broken by crevasses that melting waters soon find their way to the bottom. But a different state of affairs may have prevailed in the continental glacier. Several of these kame rivers are a hundred or more miles in length. Granting all reasonable development of sub-glacial streams, these kames can scarcely be thus accounted for. Superficial water flowing along the surface would gradually deepen its channel: when the melting had so far proceeded that the bottoms of these streams reached the moraine stuff in the lower part of the ice, the kame gravel would begin to gather on the bottoms of their channels. During the final melting, when the condition was such that few if any additional crevasses would be formed, there would be no time to extend the previously formed sub-glacial channels. The sudden floods would pass over the lowest part of the ice as they would over ground. A great and rapid northward extension of the superficial streams would result.

In discussing this paper, Mr. Upham stated that erosion does not appear in kames. They are not unfrequently a hundred feet in height: one on the borders of the Merrimac river was instanced. They appear to be gravel deposits laid down before the glacier was fully melted.

Relation of the glacial dam at Cincinnati to the terrace in the upper Ohio and its tributaries.

BY I. C. WHITE OF MORGANTOWN, W. VA.

THIS paper, in the absence of its author, was read by Professor Winchell.

In a paper read before the Boston society of natural history, March 7, 1883, Rev. G. F. Wright showed that the southern rim of the great northern ice-sheet

covered the Ohio river near the site of New Richmond, a few miles above Cincinnati; and presented the hypothesis, that one effect of this invasion of the Ohio valley by the glacial ice was to form an immense dam of ice and morainic *débris*, which effectually closed the old channel way, and set back the water of the Ohio and its tributaries, until, rising to the level of the Licking River divide, it probably found an outlet through Kentucky, around the glacial dam. The writer of the essay, after reviewing the evidence, regards Mr. Wright's hypothesis as proved beyond a reasonable doubt. He also claimed, that during the period of the continuance of the dam, the principal tributaries of the Ohio had their valleys filled with sediment carried down and dumped into them by the mountain torrents and other streams which drained the area south from the glaciated region; that subsequently, when the barrier disappeared, the rivers recut their channels through the silt deposits, probably by spasmodic lowering of the dam, in such a manner as to leave the deposits in a series of more or less regular terraces, which in favored localities subsequent erosion has failed to obliterate, though from steep slopes it has removed their every trace. The elevation of this dam at Cincinnati, as determined from the upper limit of the fifth Monongahela River terrace, would be somewhere about 625 feet above low water there in the present Ohio.

In discussing this paper, Professor Lesley said that there were two separate glacial formations to be considered, and the two could not be correlated. The ice-dam could not thus be explained. Professor Wright had discussed the subject with clearness, claiming that the dam was glacial; but at best there were only a few places in the west where the height of the ice could be measured.

The eroding power of ice.

BY J. S. NEWBERRY OF NEW YORK.

The object of this essay was to enter a protest against the theories of certain geologists who claim that glacial ice has not played an important part in the erosion of valleys. They have undertaken to deny that ice has any great excavating power. Examples of utterances of this school, the speaker said, were to be found in Prof. J. D. Whitney's Climatic changes; in papers by Prof. J. W. Spencer, on the Old outlet of Lake Erie; by Mr. W. M. Davis, on the Classification of lake basins, and the erosive action of ice; and remarks on the same subject by Prof. J. P. Lesley.

The most important heresies which had been advanced in regard to this subject were, first, the denial that there was ever a glacial period; second, if there was an ice period, it was a warm and not a cold one; third, that the phenomena usually ascribed to glacial action in the record of an ice period were generally due to icebergs; fourth, that ice has little or no eroding power, and that glaciers have never been an important geological agent. Professor Newberry proceeded, in controversion of these theories, to give the results of his extended studies of geological action in the Alps and in many different regions of the United States and Canada. These observations lead to the conclusions, 1°. That the glacial period was a reality, and that its record constitutes one of the most important and interesting chapters of geological history; 2°. That this was a cold period; 3°. That ice has a great, though unmeasured and perhaps immeasurable, eroding power; and that, in regions which they have occupied, glaciers have been always important, and often preponderating, agents in effecting geological changes.

No cautious geologist would assert or concede that *all* lake-basins had been excavated by ice, but to deny its influence in their formation would be a far greater error. The basins of our great lakes, and of many of our smaller ones, bear the traces of ice that has moved in the line, at least approximately, of their major axes. The broad, boat-shaped basins indicate the work of this same agency. The islands of Lake Erie are carved from the solid rock: their surfaces and sides, and the channels between them, are all glaciated. The plastic ice has inwrapped those islands, fitting into every irregularity, and carving, with the sand it carried, every surface. The marks of glaciation are to be seen on mountain belts from Canada to Mexico. Even at the present day glaciers are transporting enormous loads. In midsummer the Aar glacier brings down 280 tons per day; the Justedal glacier of Norway wears down, it is estimated, 69,000 cubic meters of solid rock annually. These measurements of the eroding power of two small glaciers should show the fallacy of a denial of the excavating power of ice. Dr. Newberry concluded by citing authorities on the subject.

This paper elicited the most acrimonious discussion of the meeting. Professor Lesley took exception to certain phrases in the paper which seemed to cast a reflection upon the methods of his coadjutors, — men who were conscientiously engaged in scientific investigation, and had seen reason for breaking away from the trammels of opinion formulated by Agassiz and Ramsey. For himself, he did not believe in the theory of erosive glacial processes, and he asserted that there was no good reason for believing that the basins of the great lakes were so produced. He claimed that the basin of Ontario was a Silurian valley; the basins of Erie, Michigan, and Huron, were Devonian valleys. Ice had no more eroding effect than a piece of sandpaper has upon a rough board. He believed in the eroding of water, and represented his idea of the relative power of ice and water, as follows: Ice, 1; rain-water, 10; acidulated water, 100; ice set with stones, 1,000; water set with stones, 10,000.

Professor Newberry disclaimed any intention of attacking the young men of science who were laboring in this field. He re-affirmed the positions taken in his paper. On the other hand, Professor T. Sterry Hunt declared his substantial agreement with the views of Professor Lesley. On account of the length of this debate, the five-minute rule for discussions was adopted and subsequently enforced.

Informal remarks on moraines and terraces.

BY J. W. DAWSON OF MONTREAL, AND J. W. POWELL OF WASHINGTON.

AT the opening of the morning and afternoon sessions of the geological section in its last day's work, Dr. Dawson and Major Powell made respectively some informal remarks of interest. Dr. Dawson objected to the loose significance with which the term 'moraine' had been used, and especially to the definition of it as 'detrital matter heaped up by the forcible mechanical action of ice.' He pointed out that such a definition would include work which certainly was not performed by land glaciers.

Dr. Dawson described the glacial deposits exposed along the line of the Canadian Pacific railway, from the Laurentian areas west and north of Lake Superior to the Rocky Mountains, noticing the lacustrine deposit of the Red-river valley, containing only a very few, small, ice-borne stones; the second prairie level covered with Laurentian drift from the north-east, and with an interrupted ridge of scrub material extending along the middle of it, northward from Turtle mountain. He referred to the great Missouri coteau, at an elevation of 2,500 feet, and made up of local mud, and sand, with Laurentian bowlders piled up against the higher prairie steppe; the drift on the surface of this steppe being partly Laurentian and Silurian from the east, and partly from the Rocky Mountains. He finally stated, that huge Laurentian and Huronian bowlders were placed at an elevation of more than 4,000 feet on the foot-hills of the Rocky Mountains, more than 700 miles from their original site. He did not intend to offer any explanation, as investigations into the matter were still being carried on by his son; but he wished to say briefly, that it appeared to him perfectly plain that we could not account for such phenomena as had been described, without taking into account great changes of level, or, without doubt, great submergence and remergence.

Major Powell called attention to the fact, that wholly different agencies, each acting in its own way, produced a class of geological features that went under the general name of 'terraces.' We have sea-beach terraces, lake-shore terraces, and yet another class of terraces exceedingly common in the Rocky and the Cascade mountains. The last-named class of terraces is due to a different cause from the others. Some of this class in the east have been relegated erroneously to the class of beach terraces: those which are said to dam the Ohio, and others found in the Alleghanies, have been formed by a process which can be briefly sketched.

We have a valley. It runs irregularly between bluffs and mountains. We have a force in the river which simply tends down stream; it is itself irregular, its energy depending upon its transient volume and local depth. If the region is upheaved, the river no longer keeps its old course. It seeks the line of least resistance, and may form a new flood-plain below. Then the river, for a while at least, excavates laterally instead of vertically. No longer occupying its old place in the valley, it gradually cuts a new path. But the old terrace may remain. In some places there are more than twenty systems of terraces: in a locality near Pittsburgh, there are fifty-three such systems. These the speaker regarded as chiefly due to changes in the level of the regions, — to elevations and depressions. Further explanation by the speaker was cut short under the five-minute rule.

(OTHER GEOLOGICAL PAPERS.)

The earth's orographic framework; its seismology and geology.

The 'continental type,' or the normal orography and geology of continents.

BY RICHARD OWEN OF NEW HARMONY, IND.

THESE papers were read successively, as being closely related. They refer to a well-known theory of their author, which traces the frame-work of the earth in its mountain chains. He finds such a framework running from east to west in numerous parallel ranges near the equator, and instances those of Sumatra and of South America. This he calls the 'strong girdle' of the earth: it is of mesozoic age, terminating its heights in the cenozoic age. Remotely parallel are the arctic and antarctic belts. Great braces come down to meet this girdle, having at least four ramifications in Asia, starting from the great plateau, and in America forming the great 'backbone' of the continent. The five equidistant continental trends of mountain chains often mark paleozoic belts. But the later as well as the older results tell of strong interior forces that have produced the mountains, and the central belt gives marked evidence that an intense reaction from within aided in its construction.

The similarity of the five great continents has often been the occasion of remark. They seem to have a general plan of construction, that may have been connected with their appearance as land above the ocean. The similarity extends even to their present geographical area. If we cut cross-sections from W. S. W. to E. S. E. through the geographical centre of each continent, we shall find in each case a seismic belt near one rim of the continent, and often near both rims. Thus the continent is usually basin-shaped, and comparatively low in its central area with its chief river drainage, and low near the ocean borders; rising in an eastern and western main range, with usually several parallel subordinate ridges. These eastern and western mountains converge southerly, thus assuming a somewhat irregular form, evolving usually on the west some table-land. The eastern river is usually paleozoic, with perhaps some mesozoic on the flanks and cenozoic on the ocean border. The western elevation is more commonly mesozoic in its main range, and cenozoic in the flanks or subordinate ridges. A section running north and south through the three northern continents would successively expose Cambrian, paleozoic, mesozoic, and cenozoic cuts, which would generally increase in area as we go south.

The papers of Professor Owen elicited, for the greater part, unfavorable comment. It was urged against them, that their generalizations were too broad, and that they were based rather upon closet study than actual observation. As to at least one of the continents, we know as yet far too little of its geology, especially in the interior, to frame a theory of its history and constitution.

The pre-Cambrian rocks of the Alps.

BY T. STERRY HUNT OF MONTREAL, CAN.

The writer began by reviewing the history of Alpine geology, and noticed first that speculative period when the crystalline rocks of the Alps, including gneisses, hornblendic and micaceous schists, euphotides, serpentines, etc., were looked upon as altered sedimentary strata of carboniferous or more recent times. He then traced the steps by which these views have been discarded, and more and more of these rocks shown to belong to eozoic or pre-Cambrian ages. In this connection the labors of von Hauer, Gerlach, Heim, Favre, Renevier, Lory, Gastaldi, and others, were analyzed; and reference was made to the great progress since the writer in 1872 published a review of Favre on the geology of the Alps.

The sections by Neri, Gerlach, and Gastaldi in the western, and those of von Hauer in the eastern Alps, were described; and it was shown that all these agree in establishing in the crystalline rocks four great divisions in ascending order: 1°. The older granitoid gneiss with crystalline limestones, graphite, etc., referred by Gastaldi to the Laurentian. 2°. The so-called *pietre verdi*, or greenstone group, consisting chiefly of dioritic, chloritic, steatitic, and epidotic rocks, with euphotides and serpentines, including also talcose gneisses, limestones, and dolomites, and regarded by Gastaldi as Huronian. 3°. The so-called recent gneisses of von Hauer and Gastaldi, interstratified with and passing into granulites and micaceous and hornblendic schists, also with serpentines and crystalline limestones. 4°. The series of argillites and soft glossy schists with quartzites and detrital sandstones, including also beds of serpentine with talc, gypsum, karstenite, dolomite, and much crystalline limestone. This fourth series, well seen at the Mont Cenis tunnel, is still claimed by Lory and some others as altered trias; but the present writer's view, put forth in 1872, that it is, like the preceding groups, of eozoic age, was subsequently accepted by Favre and by Gastaldi, and is now established by many observations. To this horizon belong the crystalline limestones of the Apnan Alps, including the marbles of Carrara.

The writer next recalls the fact that he, in 1870, insisted upon the existence of a younger series of gneisses in North America, alike in the Atlantic states, in Ontario, and to the north-west of Lake Superior. These, in his address before the American association for the advancement of science, in 1871, he further described under the name of the White-Mountain series, and subsequently, in the same year, called them Montalban. These rocks were then declared to be younger than the Huronian, and to overlie it; though, in the absence of this latter, it was pointed out that in Ontario and in Newfoundland the Montalban reposes unconformably upon the Laurentian. When these newer gneisses and mica-schists were first described, in 1870, there was included with them an overlying group of argillites, quartzites, and crystalline limestones; and for the whole the name of Terranovan was suggested, provisionally. But in defining, in the following year, the White-Mountain series, this upper group was omitted, and was subsequently referred to the Taconian series, — the lower Taconic of Emmons, and the so-called altered primal and auroral of H. D. Rogers, in eastern Pennsylvania.

The writer next describes his own observations in the Alps and the Apennines in 1881. He affirms the correctness of Gastaldi in referring the groups one and two to Laurentian and Huronian, finds the third, or the younger gneiss and mica-schist group of the Alps, indistinguishable from the Montalban, and regards the fourth as the representative of the American Taconian. It was maintained by Gastaldi, that these pre-Cambrian groups of the Alps underlie directly the newer rocks of northern and central Italy, forming the skeleton of the Apennines, reappearing in Calabria, and, moreover, protruding in various localities in Liguria, Tuscany, and elsewhere. The serpentines, euphotides, and other resisting rocks thus exposed, have been regarded as eruptive masses of triassic and eocene time. The writer, however, holds with Gastaldi, that they are indigenous rocks of pre-Cambrian age, exposed by geological accidents.

The uncrystalline rocks of the mainland of Italy are chiefly cenozoic or mesozoic, and the only paleozoic strata known are carboniferous, the organic forms in the limestone of Chaberton having been shown to be triassic. Triassic, liassic, cretaceous, eocene, and miocene strata are found in different localities, resting on the various pre-Cambrian groups. In the island of Sardinia, however, all these are overlaid by a great body of uncrystalline lower paleozoic rocks, in which the late studies of Bornemann and Meneghini have made known the existence of a lower Cambrian fauna, including Paradoxides, Conocephalites, and Archeocyathus, succeeded by an abundant fauna of upper Cambrian or Ordovian age.

The existence of the younger or Montalban gneiss in Sweden and in the Harz and the Erzgebirge was noticed, and to it were referred the Hercynian gneisses and mica-schists of Gümbel. The presence both in Sweden and in Saxony of conglomerates, as described by Hummel and by Sauer, wherein pebbles of the older gneiss are enclosed in beds of the younger series, was discussed, and the direct unconformable superposition of the latter upon the older gneiss, in the absence of the Huronian, was considered; evidences of the same relations being adduced from the Alps. The gneisses of the St. Gothard, as seen on the Italian slope, were also referred to the newer series; and the important studies of Stapff in this

connection were discussed. It was declared that the views put forth by the author in 1870-71, on the relations and succession of the crystalline stratified rocks in North America, and then extended by him to Europe, have been fully confirmed by the labors of a great many European geologists, as already shown. Those of Hicks, Hughes, Bonney, Callaway, Lapworth, and others, in the pre-Cambrian rocks of the British islands, were cited in support of these conclusions. It was said, that, whatever may have been the conditions under which these vast series of crystalline stratified rocks were deposited, there is evidence, in the similarity of their mineralogical and geognostical relations, of a remarkable uniformity over widely separated regions of the earth's surface, as well as of long intervals of time, marked by great foldings and disturbance, and by vast and wide-spread erosion of the successive series of rocks.

In conclusion, the writer took occasion to call attention to the important labors of the present school of Italian geologists, and their great zeal, skill, and disinterested service, as shown in the memoirs of the R. accademia dei lincei, and in the work of the Royal geological commission, including the special studies, maps, and memoirs prepared by it for the International geological congress of Bologna in 1881. The new Geological society of Italy, founded at the same date, gives promise of a brilliant future, and has already published many important memoirs.

The serpentine of Staten Island, New York.

BY T. STERRY HUNT OF MONTREAL, CAN.

THE serpentine of Staten Island appears as a north-and-south range of bold hills rising out of a plain of mesozoic rocks, which on the west side are triassic sandstones like those of the adjacent mainland, including a belt of intrusive diorite, and on the east the overlying, nearly horizontal, cretaceous marls, which are traced south and west into New Jersey. The only rocks besides these mentioned, seen on the island, are small areas of a coarse-grained granite, having the character of a veinstone or endogenous mass, and others of an actinolite rock; both exposed among the sands on the north-east shore of the island.

Mather, who described this locality more than forty years since, looked upon the serpentine as an eruptive rock, related in origin to the parallel belt of diabase which is included in the triassic sandstone to the west. Dr. Britton, of the School of mines, Columbia college, who in 1880 published, in the transactions of the New-York academy of sciences, a careful geological description and map of the island, regarded the serpentine belt as a protruding portion of the eozoic series, including serpentine, which is seen at Hoboken, on Manhattan Island, and in Westchester County, New York, — a conclusion which the writer regards as unquestionably correct.

The appearance of isolated hills and ridges of serpentine is common in other regions, and is by the writer explained by the consideration that this very insoluble magnesian silicate resists the atmospheric agencies which dissolve limestones, and convert gneisses to clay; the removal of which rocks leaves exposed the included beds and lenticular masses of serpentine. Similar appearances are seen in many parts of Italy, where ridges and bosses of serpentine are found protruding in the midst of eocene strata, and have hitherto by most European geologists been regarded as eruptive masses of tertiary age. The problem is there often complicated by the fact that subsequent movements of the earth's crust have involved alike the older crystalline strata (of which the serpentines form an integral part) and the unconformably overlying eocene beds; faulting and folding the latter, and even giving rise to inversions by which the newer rocks, overturned, are made to dip towards and beneath the ancient crystalline masses. This the writer illustrated by reference to localities recently examined by him in Liguria and in Tuscany, where this relation of the serpentines had already been pointed out by Gastaldi. The structure in question was declared to be analogous to that presented by similar foldings and overturns to be seen along the western base of the Atlantic belt throughout the Appalachian valley.

The speaker further alluded to the fact, that, although the sub-aërial decay of serpentine was far less rapid than that of most other rocks, it had not escaped this process; and described the decayed layer on portions of the Staten Island serpentine hills, including a chromiferous limonite segregated from the decayed serpentine. This was a slow pre-glacial process, and in the subsequent erosion of the serpentine ridges the decayed layer has been in parts entirely removed. The details of this decay, and its relations to the limonite, and to glaciation in this locality, have been described by the writer in an essay on the decay of rocks, to appear in the *American journal of science* for September, 1883. He gratefully acknowledged his personal obligations to Dr. Britton for the many facts contained in his memoir and map, as well as for personal guidance during a late visit to Staten Island.

The equivalent of the New-York water-lime group developed in Iowa.

BY A. S. TIFFANY OF DAVENPORT, IO.

THE author stated, that the upper Silurian rocks of Iowa had hitherto been classed wholly as of the Niagara limestone. There has, however, been some dispute as to the magnesian buff-colored limestone of the Le Claire and Anamosa quarries. Such disputes must, of course, be settled by the fossils; but he had been for more than twelve years seeking organic remains in that formation, without success until February of last year, when he found them in considerable quantities. Specimens of the fossils were exhibited. Mr. Tiffany considered that they gave conclusive evidence of belonging to a group higher in the scale than the fossils of the Niagara limestone, that their affinities were with those of the water-lime group of the lower Helderberg, and that the identity of many species had been determined.

Clay pebbles from Princetown, Minn.

BY N. H. WINCHELL OF MINNEAPOLIS, MINN.

This paper was accompanied by an exhibition of specimens. The pebbles were of various shapes and sizes, several of them somewhat cylindrical. Outside, they are composed of fine sand and gravel; inside, they consist wholly of a fine sedimentary clay, such as is deposited by standing water, and contain no interior pebbles. Professor Winchell had compared these with pebbles found in till-deposits, and with various others, without finding any thing exactly similar.

Professor Newberry examined the pebbles, and admitted that they were not exactly like any that he had seen, but he thought they bore a general resemblance to pebbles found throughout the range of geological strata wherever there is a bed of sandstone capped by clay. Professor Claypole claimed to have seen similar specimens in Pennsylvania deposits.

The 'earthquake' at New Madrid, Mo., in 1811, probably not an earthquake.

BY JAMES MACFARLANE OF TOWANDA, PENN.

After dwelling upon the fact, that the locality of the alleged earthquake was not the seat of any apparent volcanic action, the author proceeded to state his view that the event in question was due to a different cause. He claimed that the locality was underlaid by cavernous limestones of the St. Louis group. He believed that what took place was a subsidence, due to the solution of underlying strata. He alluded to the descriptions afforded by Humboldt and Lyell, the latter having visited the locality, and given it a careful examination. The inhabitants described it as a convulsion, taking place at intervals during several months, creating new lakes and islands, changing the face of the country. The graveyard was precipitated into the Mississippi river; forest-trees were tilted in all directions; vast volumes of sand and water were discharged on high.

The author claimed that the long continuance of such phenomena, which lasted for several months, was an evidence that they proceeded from mere subsidence, and not from earthquake shock. In respect to the geology of the region, he stated that New Madrid and its vicinity rested on tertiary or quaternary strata. Underlying sub-carboniferous formations are represented near the borders of the depression. The sinking of a shaft brought to light coal, or coal-shales; also there were coaly shales found in the crevices and sink-holes thirty-five years after the so-called earthquake.

This paper elicited strong expressions of dissent from several members. Professor Cox declared that there were no sub-carboniferous rocks in that locality, no caverns, no soluble limestones underlying the surface. The shocks were sudden. There was great destruction of life. No mere subsidence can account for what actually happened. A question as to the truthfulness of the reports from that region brought out very contradictory opinions in the discussion. Professor Cox, who had personally examined the scene of the occurrences, declared that he had found evidences of great disturbance. Professor Nipher suggested that the position of the trees, whether upright or not, which were alleged to be at the bottom of Reelfoot Lake (a lake formed at the time of the earthquake), would help to determine whether a subsidence, or an earthquake, had taken place. Some doubt was expressed as to whether any submerged trees were there. To these doubts and queries, Professor Cox was able to give a definite answer: he had seen the trees still upright beneath the water.

Comparative strength of Minnesota and New-England granites.

BY N. H. WINCHELL OF MINNEAPOLIS, MINN.

Having had recent occasion to test the qualities of the building-stones of Minnesota, the author subjected them to the usual tests of crushing, using for this purpose specimens of two-inch cube. The specimens included sandstones, limestones, granites, and trap-rocks, and numbered about 100. Great care was taken in preparing them accurately. They were sent to Gen. Gillmore at Staten Island, and there subjected to the tests, which were applied by crushing the samples, one in the direction of the schistose structure and one across it. The following were the results with twenty samples of Minnesota granites:

Kind of stone.	Location of quarry.	Position.	Strength in pounds.	
			Of sample.	Per cubic inch.
Dark trap-rock, massive melaphyr	Taylor's Falls, Chisago county	On bed	105,000	26,250
		On edge,	105,000	26,250
Dark trap-rock, from a dyke	Fischer's creek, n'r Duluth, St. Louis county,	On bed	105,000	26,250
		On edge,	105,000	26,250
Gray gabbro, massive, fine,	Rice's Point, Duluth, St. Louis county,	On bed	109,000	27,250
		On edge,	105,000	26,250
Red, fine sienite	Beaver Bay, Lake county,	On bed	106,000	25,000
		On edge,	103,000	25,750
Red quartzose sienite	Watab, Benton county	On bed	103,000	25,750
		On edge,	103,000[1]	25,750
Red quartzose sienite	East St. Cloud, Sherburne county	On bed	112,000	28,000
		On edge,	105,000	26,250
Red quartzite	Pipestone City, Pipestone county	On bed	111,000	27,750
		On edge,	108,000	27,000
Massive gray quartzose sienite	East St. Cloud, Sherburne county	On bed	105,000	26,250
		On edge,	103,000	25,750
Fine-grained gray sienite	East St. Cloud, Sherburne county	On bed	112,000	28,000
		On edge,	105,000	26,250
Fine-grained gray sienite,[2]	Sauk Rapids	On bed	86,000	21,500
		On edge,	100,000	25,000
Average of	twenty samples,		104,800	26,675

Allowing for eleven per cent difference between processes of crushing between steel-plates and between wooden cushions, this gives an average for Minnesota granites of 23,318 pounds.

[1] Estimated. [2] Probably imperfect sample.

The following are the records of tests of New-England granites: —

Kind of stone.	Location of quarry.	Position.	Strength in pounds. Of sample.	Per cubic inch.
Blue	Staten Island, N.Y.	On bed	89,250	22,315
	Fox Island, Me.	" "	59,500	14,875
	Dix Island, Me.	" "	60,000	15,000
Dark	Quincy, Mass.	" "	71,000	17,750
Light	Quincy, Mass.	" "	59,000	14,750
Flagging	Hudson River, N.Y.	" "	53,700	13,425
	Cape Ann, Mass.	On bed	59,750	14,937
Porter's rock,	Mystic River, Conn.	On bed	72,500	18,125
Gray	Stony Creek, Conn.	On bed	60,000	15,000
Gray	Fall River, Mass.	On bed	63,750	15,937
Bluish-gray	Keene, N.H.	On bed	41,000	10,250
Bluish-gray	Keene, N.H.	On bed	51,500	12,875
	Millstone Pt., Conn.	" "	64,750	16,187
	Greenwich, Conn.	" "	45,200	11,300
Niantic river,	New London, Conn.	" "	50,000	12,500
Niantic river,	New London, Conn.	On edge,	56,700	14,175
	Vinalhaven, Me.	" "	52,600	13,150
	Vinalhaven, Me.	" "	67,000	16,750
	Westerly, R.I.	On bed	58,750	14,689
	Westerly, R.I.	On edge,	59,750	14,937
Average of	twenty granites		59,785	14,946

After discussing several supposable causes of error, and showing that they could not have applied to the present case, the author proceeds to suggest causes why the Minnesota granites may be stronger than those of New England. He thinks those of the west may have been less changed by decay. The lateness of the glaciation to which they were exposed may have left them comparatively fresh through the recent removal of a considerable thickness. On this point we shall be more certain when the glacial moraines have been fully traced from east to west, and the western analogues are determined.

The singing beach of Manchester, Mass.

BY A. A. JULIEN OF NEW YORK AND H. C. BOLTON OF HARTFORD, CONN.

SANDS were taken from the so-called 'singing beach' on the coast of Massachusetts, near Manchester-on-the-Sea, and subjected to microscopical examination. In this beach, the felspathic rocks are intersected by numerous dykes of igneous rocks, among which porphyritic diorite is noticeable. The phenomenon which gives rise to the name of the beach is confined to the portion of sand lying between the water-line and the loose sand above the reach of ordinary high tide. Portions emit the sound; but closely contiguous areas fail to do so, or answer feebly. The sounding sand is near the surface; at the depth of one or two feet it ceases, perhaps because of moisture. The sound is produced by pressure, and may be likened to a subdued crushing; it is of low intensity and pitch, is not metallic nor crackling. It occurs when the sand is pressed by ordinary walking, increases with sudden pressure of the foot upon the sand, and is perceptible upon mere stirring by the hand, or even plunging one finger and removing it suddenly. It can be intensified by dragging wood over the beach.

The authors review and cite very fully the literature of the subject, giving in full a description of the singing sands of the island of Kaui, one of the Hawaiian group. That gives a sound as of distant thunder, when any thing of weight is dragged over it. Dampness prevents the sound. That sand is calcareous. Hugh Miller cites similar instances at Jebel Nakous in Arabia Petrea, and Reg Rawan near Cabul. Those are silicious sands. The sounds were a sort of humming.

In Churchill county, Nevada, a similar phenomenon is described with regard to a sand-hill, as like the sound of telegraph-wires when wind blows them.

The authors also review and characterize the various sands of different mineral origin.

To explain the sonorous peculiarity of the sand, several theories are considered. That of equality, or of the unequal size of the grains, is rejected. Cellular structure has been supposed, but is not found in the present instance. Effervescence of air between moistened surfaces does not apply to this case. Sonorous mineral, such as clinkstone, is not present. There is no evidence of electrical phenomena being concerned. The hypothesis adopted is that the sand, instead of being, as ordinarily, composed of rounded particles, is made up of grains with flat and angular surfaces. In the present instance, the plane surface of felspar is apparent in many of the grains. Probably a certain proportion of quartz and felspar grains is adapted to give the sound, while less or more of either component would fail of the result.

Dr. Bolton has himself examined a sand of similar quality, on the island of Eigg in the Hebrides, and has described its properties. That is largely calcareous. Its constitution is a mixture of large and small grains, the larger ones being rounded quartz. Many small, angular fragments of quartz are also contained, and many dark granules of chert, the last being about three or four per cent of the whole, and having a cellular structure.

It is concluded, that the sound is produced either by the intermixture of grains having cleavage planes, or of grains with minute cavities. The paper ends with a table of the physical structure of the sands of many localities.

(*PALEONTOLOGICAL PAPERS.*)

Preliminary note on the microscopic shell-structure of the paleozoic Brachiopoda.

BY JAMES HALL OF ALBANY, N.Y.

IN the earlier studies of the Brachiopoda, the numerous species were referred to few generic terms, determined from their perforated apex and external form, and later from the study of the interior as these became known. The author said, that from time to time, as these characters had become known to him from the study of large collections, he had found it necessary to propose the separation of eighteen new generic forms from those previously known in this class of fossils. Other authors had also proposed new generic terms, until the list had become many times greater than it was twenty-five years ago.

While the interior structure of the hinge, and the muscular and vascular markings, were now pretty well known for most of the generic forms in use, comparatively little attention had been given to the minute structure of the shell. Little more had been done than to show that some forms possessed a punctate and others a fibrous texture.

The study of this structure had been commenced by him many years ago, but he had been thwarted in his efforts to procure the required cutting and polishing of specimens of the shells for microscopic study. He had now been able to obtain such thin slices of the shell as were required for this purpose, and had already several hundred slides prepared for the microscope.

A few of these only were shown, exhibiting the shell structure of as many genera. A considerable number of photographs had been made, illustrating, in a very satisfactory manner, the minute structure of each one, enlarged to twenty diameters. The photographs exhibited were illustrations of several species of Orthis, Leptaena, Strophomena, Strophodonta, Chonetes, etc.

The study of this shell-structure has shown very satisfactorily, what was partially known before, that the genus Orthis, as now defined and constituted, includes very heterogenous material. External form, hinge characters, and interior muscular impressions, have been the chief guide; and yet forms have been included under this genus, showing widely different muscular markings. On further microscopic study, it has been found that these differences in form of muscular imprints are accompanied by important differences in the shell structure.

These differences may be noted in the illustrations presented, where the shell of Orthis biforata, O. borealis, O. tricenaria, O. occidentalis, O. flabella, are non-punctate and coarsely fibrous. Orthis (?) strophomenoides is, like Streptorhynchus, fibrous. Orthis subquadrata has, like O. occidentalis, a few large punctae.

In the second group, Orthis testudinaria, O. Vanuxemi, O. perveta, O. penelope, O. elegantula, O. clytie, and O. hybrida, have one or more rows of punctae to each ray, the rows well defined, and the intermediate shell finely fibrous.

The third group, consisting of Orthis multicostata of the lower Helderberg, O. jowensis of the Hamilton group, O. tulliensis of the Tully limestone, O. impressa of the Chemung group, are highly punctate with a fine fibrous texture of the shell substance.

The punctae usually come out along the summits of the radiating striae or plications of the shell. In some species the minute tubes perforating the shell, and producing these punctae, bifurcate and diverge before coming to the external surface of the shell.

This difference in shell structure, in forms known as Orthis, will require a separation of the species into groups based upon the shell structure, and character of muscular impressions. Already we see that the shells of compact fibrous texture have a form of muscular impression quite unlike those with the punctate structure; and we shall probably find that all the interior modifications of the muscular system are accompanied by differences in the microscopic structure.

This method of determining the shell structure, in cases where the specimens may be imperfect, and thereby enabling the determination of obscure or fragmentary material, and its geological relations, will be of much importance to the geologist.

The structure of the shell in Strophomena is closely fibrous, with distant large punctae. In Strophodonta, the punctae are more numerous. In Chonetes, the punctae are large, and arranged parallel to the radii, having a pustulose aspect.

In many other forms, the punctate texture of the shell is characteristic, and of importance in the determination of the generic forms.

The physiological significance of this peculiar shell structure will be considered upon some future occasion, illustrated by more numerous examples.

Rhizocarps in the paleozoic period.

BY J. W. DAWSON OF MONTREAL, CAN.

The author referred to a previous memoir, entitled 'Spore-cases in coal,' published in 1871. This described fossil remains in a shale from the Erian formation at Kettle Point, Lake Huron, supposed to be on the horizon of the Marcellus shale of New York. The remains are minute brownish discs scarcely more than one-hundredth of an inch in diameter. They were recognized as probably spore-cases or macrospores of some acrogenous plant. The shale also contains vast numbers of granules, which may be escaped spores or microspores. In 1882 Dr. Dawson's attention was called to the discovery of similar bodies in vast numbers in the Erian and lower carboniferous shales of Ohio. The discoverer, Professor Orton, regarded these bodies as spore-cases, and as the chief source of the bituminous matter in those shales. Professor Williams found similar bodies in the Hamilton shales of New York; and Prof. J. M. Clarke, in the Genesee shale and in the corniferous limestone. The last named are of larger size than the others.

No certain clew had been thus far afforded to the affinities of these widely distributed bodies. But last March, specimens were found in the Erian formation of Brazil, by Mr. Orville Derby, which threw new light on the subject, containing as they did, along with the Sporangites, abundant fronds of Spirophyton. The Sporangites of Brazil resemble in every respect the involucres or spore-sacs of modern rhizocarps, and especially the sporocarps of the genus Salvinia.

Dr. Dawson describes with technical exactness two leading types which he has named provisionally Sporangites braziliensis and S. bilobatus. The paper offers the suggestion that these plants, now so insignificant, culminated in the paleozoic age, and, occupying the submerged flats of that period with abundant vegetation, produced a great quantity of the bituminous matter found in resulting beds. A rich rhizocarpean vegetation in the early paleozoic and

eozoic ages may have preceded the great development of acrogens in the later paleozoic.

In the discussion which followed, Dr. Dawson disclaimed any intention to assert that the Sporangites were the sole source of the bituminous matter.

Rensselaeria and a fossil fish from the Hamilton group of Pennsylvania.

BY E. W. CLAYPOLE OF NEW BLOOMFIELD, PENN.

THE Hamilton sandstone of Pennsylvania is found in ridges just before we come to the Blue Mountains. The sand tapers off from a centre in these ridges both ways. At places it is eight hundred feet in thickness, some of it quite hard and flinty. Perhaps this sand was left by rivers; but, at all events, where it is missing it must have been cut away by erosion. The author believed that an ancient river had occupied nearly the place of the present Susquehanna, but running in an opposite direction, — to the north, — and probably debouching where the city of Harrisburg now stands. That locality had previously been below the sea: it was raised so as to become dry land through which this river runs. That land and that river again sank slowly. Then the sunken land received sand from the river. Afterward this region became the bed of a sea. It is a fan-shaped deposit, thickening toward the centre of the fan.

The author exhibited a model of a fish whose remains were discovered in this sandstone. He also showed specimens of alleged Rensselaeria found in the Hamilton sandstone. The latter were shown to Prof. James Hall, during the reading of the paper. Mr. Claypole thought them identical with the Rensselaeria of the Oriskany sandstone, there being a difference of a thousand feet between the two horizons; and he believed this the first instance of such discovery. The strata were tilted on edge in the locality where the fossils were found. Mr. Claypole made a diagram of the geological structure of the region. The fossils were in the middle of the sandstone, which is six hundred to eight hundred feet thick. A Spirifer very much like S. formosa is found there in great quantities.

Professor Hall, after a brief examination, said that anybody was excusable for supposing the fossils to be Rensselaeria. The differences between them and the Oriskany fossil were slight though well marked. Professor Hall described some of these differences, and Mr. Claypole acknowledged that a certain V-shaped groove was wanting in his specimens. Professor Hall thought that possibly the fossils should be referrred to Amphigenia, which had many similarities to Rensselaeria. Professor Newberry thought the fish fossil new.

A large crustacean from the Catskill group of Pennsylvania.

BY E. W. CLAYPOLE OF NEW BLOOMFIELD, PENN.

OF this fossil the author exhibited a cast. It showed no evidence of fish structure. Its apparent affinities were with the king crab, yet it was not a true Limulus nor even a limuloid. A cast in gutta-percha was also shown, which better exhibited the markings. There was a resemblance in the fine surface-marks to Eurypterus. But the eurypterids, with a single exception, were all found in strata vertically distant six thousand feet.

Professor Hall said that the eurypterids were widely distributed. They were found in the coal-measures, in the Waverly sandstone, and perhaps — though that was not quite certain — in the Portage group.

Animal remains from the loess and glacial clays.

BY WILLIAM McADAMS OF ALTON, ILL.

THE drift clays proper at Alton, Ill., had a maximum thickness of about one hundred feet, and the bluff clays were nearly of the same thickness. These clays were remarkably rich in animal remains, such as teeth and bones, attached to calcareous nodules or claystones. Remains of thirteen different species, now perhaps all extinct, had been found. The rodents were well represented in the bones of seven species, including three or more beavers and some gophers. Nearly seventy teeth were found in the quaternary deposits, a majority of them in a single quarry.

A new vertebrate from the St. Louis limestone.

BY WILLIAM McADAMS OF ALTON, ILL.

ONE of the groups of subcarboniferous limestone is quarried extensively near Alton and St. Louis. It lies beneath the coal, and in some places the coal rests directly upon it. A number of vertebrate remains have been found in one of the quarries near Alton. Specimens were shown by the author of the paper. In the judgment of Professor Newberry, the fossils shown were the bones of some large fish. One appeared to be the mandible or dental bone of the lower jaw. Without pronouncing a final opinion, he would say that it bore a general resemblance to a group of fossil fishes in which the teeth were inserted in sockets; but the animal itself was large and hitherto unknown.

List of other papers.

The following additional papers were read in this section, some of them by title only: Thermal belts, by *J. W. Chickering*. The Hamilton sandstone of middle Pennsylvania, by *E. W. Claypole*. Evidences from southern New England against the iceberg theory of the drift, by *J. D. Dana* [this paper will appear in full in SCIENCE]. Topaz and associated minerals from Stoneham, Oxford county, Me.; Colored tourmalines and lepidotite crystals from a new American locality; A note on the finding of two American beryls; Andalusite from a new American locality; On a white garnet from near Hull, Canada, — by *G. F. Kunz*. The genesis and classification of mineral veins, by *J. S. Newberry*.

PROCEEDINGS OF SECTION F.—BIOLOGY.

ADDRESS OF W. J. BEAL OF LANSING, MICH., VICE-PRESIDENT OF THE SECTION, AUG. 15, 1883.

AGRICULTURE: ITS NEEDS AND OPPORTUNITIES.

INSTEAD of presenting a summary of the progress made in biology during the past year, I have chosen, rather, to speak of the 'Needs and opportunities of agriculture,'—a subject which has heretofore scarcely been mentioned at the meetings of this association. Within the past few years the progress of agriculture, which I use in its broadest sense, has been greater than ever before. This may be attributed to a variety of causes; such as the general thrift and intelligence of our people, and the advancement of science.

Many agricultural schools have been established, experiment-stations organized, the rural press has been much improved in quality and quantity, clubs and societies are flourishing, and thousands of granges have helped to stimulate thought and investigation. Though there is much chance for improvement, the U. S. department of agriculture in several of its sections has done excellent work. It is true, and it is strange that it should be true, that, until within a comparatively recent period, but very little of the best thought, even of civilized nations, has been devoted to subjects intended to advance agriculture.

Columella, eighteen hundred years ago, keenly felt the want of more thought in agriculture when he said, "Husbandry alone, which, without doubt, is next to, and, as it were, near akin to, wisdom, is in want of both masters and scholars. Of agriculture, I have never known any that professed themselves either teachers or students." Many of our states have freely appropriated money to conduct surveys in geology, mining, with a little attention given to zoölogy and botany, not neglecting to provide liberally for coast-surveys.

The nation, considering its age, has also been very generous with money in support of surveys of various kinds, including, also, anthropology, construction of lighthouses, improving rivers and harbors, investigating the supplies of fish, and even astronomy has been generously supported. It is true that some of this work performed by the government has been very poorly done, and has been enormously expensive; but the methods of work are improving.

This munificence of the United States in support of science is encouraging, and, as far as it goes, speaks well for the country and our law-makers. Doubtless, in many cases, the close connection with politics is one great hindrance to successfully conducting investigations in science for the government. The chances of losing positions are often too great to make them desirable, especially to persons who dislike political contests. Frequent changes are fatal to good, long-continued work.

Notwithstanding the large sums of money expended by our national and state governments in support of science, but a small sum, considering its importance, has been appropriated in the interest of agriculture. Even private gifts have gone to endow literary colleges, schools of physical or natural science, astronomical observatories, public libraries, and not to endow something which is directly intended to encourage agriculture. The men like Lawrence, Sheffield, Smithson, Peabody, Washburn, Swift, Stevens, are numerous, but not numerous enough. All honor to the noble names of those who have so generously contributed to the advancement of science.

To illustrate the hesitancy of men to bequeath money for the promotion of agriculture, I take the following from an address given by President T. C. Abbot:—

"I met a very pleasant and intelligent gentleman, who, from his large wealth, was about to give some sixty or seventy thousand dollars for the advancement of higher education. He had been for some years, and was still, the president of a state agricultural society. He was a farmer. Did he then endow some chair of agriculture, or agricultural chemistry, of veterinary science, of horticulture? Did he fit out an experiment-station to analyze fertilizers, to study the value of cattle-foods? Did he establish an agricultural library? None of these. He found the science that was the most advanced of any, the one that government supports at a great expense from the public treasury. This farmer gave his thousands to endow another workshop of astronomy."

Yet, even in respect to private endowment, there are approaching signs of better days for agriculture. A few far-seeing men have observed the needs of this interest, and have set a noble example by giving of their wealth bounteously. Cornell, Bussey, Purdue, Valentine, Storrs, in this country, are names which will long be honored for liberal gifts in the interest of agriculture. They showed great sagacity, and not a little originality, by placing endowments in a new field, where gifts are few, and the opportunities for good are boundless. It is hoped that these illustrious examples will stimulate others to make similar bequests.

Where agriculture thrives, there we always find a prosperous people. She needs more trained minds to work in her interest. With better thought would come great and needed improvements in the agricultural department of the nation. It lacks means, strength, and stability.

The matter of plans, and the naming of a competent director of the geological survey, was referred to the National academy of sciences, whose suggestions the government authorities sought and adopted.

The same body, or the standing committee of this association, or the members of the Society for the promotion of agricultural science, would be amply competent to name a good man for commissioner of agriculture. Such a plan would throw the position more out of politics, and it would be more likely to

run smoothly on, like the work of the Smithsonian institution.

Greater permanency would tend to make the department more efficient, and help it to co-operate with the agricultural departments of the several states and the agricultural colleges and experiment-stations.

The leading object of these remarks is to call the attention of those who are working for the advancement of pure science to the great needs of agriculture, the grand opportunities for making discoveries, and the lasting gratitude which such workers are likely to receive from the people. Of course, we grant that all science is valuable, that much of pure science has a practical bearing, that no one can foretell what practical results may be reached by investigations in pure science; still there is a tendency among scientific men to ignore economic science.

I will illustrate my meaning. The U. S. signal-service is generally supposed to have been established in the interest of science, with the avowed intention, also, of benefiting navigation. The benefits in these respects are certainly worth all they cost, but these are not all the benefits which the service should recognize.

I note the following as given by Dr. R. C. Kedzie some months ago, to illustrate the tardiness of science and the government to promptly grant assistance to the interests of agriculture

"No industry, except navigation, is so completely at the mercy of the weather as agriculture, in its widest sense. In the magnitude of the interests thus threatened, agriculture outweighs all others in importance. Indeed, without the sustaining influence of agriculture, commerce itself would vanish like the dew of morning. Timely warnings of impending meteorological dangers might be given by the signal-service, which would be of incalculable worth to agriculture."

He illustrates the subject by referring to the protracted rainy weather during the wheat-harvest of 1882, in Michigan, where the loss was very great. "The approach of a protracted storm was known for days before the damage was done. If specific warning had been given our farmers at that time, most of the wheat might have been safely housed, and the farmers of Michigan saved from a loss of more than $1,000,000. The damage inflicted in this way is not isolated and exceptional."

At length the growers of cotton and tobacco in the south, and of cranberries in New Jersey, have been recognized by the government, and warnings of approaching frosts have been promptly given. "The general government, through the signal-service, should hold the shield of its protection over land as over sea, over corn-field as over tobacco-plant, over hay-field as over cranberry-marsh, over wheat-field as over cotton-plantation, over orchards and vineyards, and the cattle upon a thousand hills and prairie leas. Why not extend this work into wider fields by doing for the *producer* what it has so well done for the *carrier?*"

The opinion seems to be too prevalent that few experiments in agriculture are worth attempting, unless it be those conducted by a chemist. This is by no means the case, though it is true that none but a chemist is capable of making those of a certain nature. A physicist will still find in the soil much to interest him, and there is, no doubt, a chance to make discoveries valuable to agriculture.

With regard to the great importance of investigations and united action concerning the control of various plagues of our domestic animals, we should suppose no one would give a dissenting voice. Some valuable investigations have been made concerning the cause and nature of these diseases, among the most interesting of which, it seems to me, are the experiments made by Dr. Salmon in using an attenuated virus for inoculating animals, and inoculating again and again with a stronger virus those not affected by the attenuated virus. If the subject of animal plagues and the means of controlling them were fully discussed at meetings of this association, it would tend to allay prejudice, enlighten the minds of our citizens, and stimulate our law-makers to action. That there is need of a more general knowledge of this subject, I quote from a recent article by Professor Law in the proceedings of the Society for the promotion of agricultural science. "The present agitation on behalf of legislation for the extinction of this lung plague in America began actively in 1878, and, notwithstanding that the subject has been continually before federal and state legislators for four years, but little real progress has been made. Among the drawbacks that may be specially named is the ignorance of legislators, of executives, and even of electors, on this subject."

In learning how to economically feed domestic animals, there is a great opportunity for investigation. There is much of interest and value to be learned in reference to the causes of fluctuation in weight of animals which are carefully fed and watered in a uniform manner.

Concerning the great need of continued and increasing efforts to investigate our injurious and beneficial insects, I need say but little; for the subject has been kept before the people, and the people are always interested to know something about an insect as soon as it injures their crops, or causes them trouble in any way. There is especially much need of more experiments to find better remedies for injurious insects. Attention to this portion of the subject cannot fail to meet with some degree of success. Success here is sure to win the gratitude of every one engaged in agriculture. Success in finding good, cheap, and safe remedies for injurious insects will tend to make science popular, and make endowments for research much easier and more frequent than ever before.

I need hardly add, that he who finds or breeds a race of honey-bees which is hardier, more industrious, longer-lived, quieter, possessed of longer tongues, and, last but not least, possessed of blunter stings, with less inclination to use them, — he who can succeed in any or all of these objects is entitled to rank

with the man who shall cause two blades of grass to grow where only one grew before.

The U. S. commission on fish and fisheries is an example of good scientific work, with prospects of early returns in the form of an increase in knowledge and a large increase in the supply of fish. A somewhat similar work, conducted by Prof. S. A. Forbes of Illinois, is in progress, where the object of the survey is to inquire into the food of birds and the food of fishes.

Some valuable scientific work of an economic nature has been done in connection with the tenth census, conspicuous among which is that performed by Prof. C. S. Sargent, in the study of forestry.

Botanical explorers in every land have repeatedly and liberally contributed plants of economic importance to the horticulturist, — a few new fruits, but more especially flowers and foliage-plants. An occasional contribution has been made to agriculture in the form of plants which promised to be of value for seeds or forage, or for some other purpose.

I have often been surprised that more attempts had not been made to secure the introduction of some new foreign grasses, and test them to ascertain their value for meadows and pastures. To be sure, grasses from western Europe have been tried; but we need others.

More than twelve years ago this idea appeared in my address on grasses, as given before the Northwestern dairymen's association, where the advice was given to get other grasses from Japan, China, central Asia, and the dryer portions of South America. The cereals and pasture-grasses, the world over, are of more value to man and his domestic animals than all other plants taken together; yet the list of pasture-grasses now generally sown in any state can be counted on the fingers of one hand. In Great Britain, where much attention has been given to the subject, twenty-five or thirty species are much cultivated. It is hard to give all the reasons why so few grasses are employed in this country; but the fact remains, that few are cultivated. The grass family is a large one, containing from thirty-one hundred to four thousand or more species. They are widely distributed in nearly all parts of the habitable globe, in every soil, in society with others, and alone. This does not convey an adequate idea of their value in unwooded regions, because the number of individuals of several of them is exceedingly large.

I have recently found the following in the *American agriculturist* for 1858, a statement probably made by Dr. Thurber. "A dozen sorts, probably, cover nineteen-twentieths of all the cultivated meadow-land from Maine to Texas. It can hardly be supposed that so limited a number meets, in the best manner possible, all the wants of so great a variety of soil and climate. This is one of the pressing wants of our agriculture. Experimental farms are needed where the value of new grasses and kindred questions can be determined. A single new grass, that would add but an extra yield of a hundred pounds to the acre, would add millions of dollars annually to the productive wealth of the nation."

Still farther back, in 1853, the late I. A. Lapham of Wisconsin expressed similar views; and still longer ago, in 1843, forty years ago, in a prize-essay, J. J. Thomas said, "The great deficiency in the number and variety of our cultivated grasses has been long felt by intelligent cultivators." In this subject, but very slow progress has been made in forty years.

In the extensive unwooded regions west of the Mississippi, the native grasses afford much pasture; but many of them start very late in spring, and stop growing early in autumn. They do not completely occupy the ground: they are easily stamped out by the hoofs of cattle and sheep. Some of the tame grasses will thrive better, and afford much more pasture.

In Science, vol. i. p. 186, of this year, Prof. N. S. Shaler refers to this subject. He says, "It seems possible to improve this pasture by the introduction of other forage-plants indigenous to regions having something like the same climate. The regions likely to furnish plants calculated to flourish in a region of low rainfall include a large part of the earth's surface. Those that would succeed in Dakota are not likely to do well in Texas or Arizona. For the northern region, the uplands of northern Asia or Patagonia are the most promising fields of search; while for the middle and southern fields, the valley of the La Plata, southern Africa, Australia, and the Algerian district, may be looked to for suitable species." He recommends three experiment-stations, — one in Nebraska, one in Texas, and one in Arizona.

In this connection, when we remember that exotic plants often thrive better than natives, we see what a vast field lies ready for experimenting with the grasses.

Grasses look much alike to all who have not closely studied them; so that farmers — in fact, none except botanists are likely to attempt experiments. This is a strong reason why the state and national governments should assist agriculture in an undertaking which seems so fruitful of good results within a short time, at so trifling an expense. Expeditions are sent at great expense to explore polar seas, with a view to slightly extending our knowledge of a barren portion of the earth's surface. Large sums are employed to fit up in magnificent style, and send to the remotest parts of the earth, expeditions to spend a few minutes in observing an eclipse or a transit of Venus. Would the sending of competent persons around the earth in search of better grasses be an undertaking less praiseworthy?

The men who control the Northern Pacific railway were enterprising enough to see that a complete economic survey of the adjacent territory would help the sale of their lands. Among other things, the grasses will be carefully examined.

For the past ten years the writer has been testing, in a small way, some hundred and fifty species of grasses. These, with few exceptions, are natives of the eastern United States and western Europe. I am fully convinced that further experiments, carefully made on a larger scale in several portions of our country, will be quite sure to result in great gain to agriculture.

Grasses suitable for the western prairies, to take the place of those which will be rapidly stamped out by close feeding, are sure to be found even without the aid of the government; but greater time will be required.

Prof. E. M. Shelton of Kansas agricultural college has probably done more than any one else in the west to test grasses and clovers, and diffuse information in regard to the results, which are most gratifying. At nearly all gatherings of farmers in the west, this question of new grasses is a prominent topic of discussion.

Wherever irrigation has been well tried, especially on land which is light and well drained, the results have been quite surprising, converting a dry, hungry meadow into a little oasis. Such a meadow is the triumph of agricultural art.

One of the most remarkable results of irrigation, as viewed by a scientific man, is this: the list of grasses will not remain the same, or maintain the same proportion. The bad grasses will nearly all die out, or improve in quality; while the best ones will rapidly increase. And again: experiments in England have shown that irrigation causes many herbaceous plants, distinct from grasses, such as plantain and buttercups, to give place to good grasses. Docks are not diminished by irrigation. The best grasses are a sign of good land in fine condition. Such grasses are hearty feeders, and are most sensitive to good treatment. In a well-managed meadow, irrigation in four years increased the value threefold.

Solon Robinson long ago expressed the view, that, if the streams of Connecticut were properly utilized in irrigating the soil, they would be more productive in value than by turning all the water-wheels of the state. More experiments in irrigation are much needed in this country.

Baron J. B. Lawes, a most renowned experimenter in agriculture, possessed an old pasture having been in permanent grass over a century. No fresh seed of any kind was sown during this period. For some seven years or more, he experimented by applying to this old pasture, on different plats, twelve different kinds of manures. The results were very interesting and gratifying. "The manures, which much increased the produce of hay, at the same time very much increased its proportion of graminaceous herbage. The total miscellaneous herbage (chiefly weeds) was the most numerous in kind, and nearly in the greatest proportion, on the *unmanured* land, — viz., sixteen per cent, — while on the manured plat it decreased to two per cent. Every description of manure diminished the number of species and the frequency of occurrence of the miscellaneous or weedy herbage. A few weeds were increased by the manures, such as Rumex and Achillaea."

"The plants of a meadow," in the words of the *Agricultural gazette*, "live in harmony on the unmanured open park, having nothing to fight for in a state of nature; but toss them a bone, ground fine, or any other choice bit, and their harmonious companionship terminates at once. Every act of improved cultivation occasions instant war. A grass likes the best that can be got. It will swallow soda, but not when it can get potash. As a general principle, all manures tend to drive out the weeds by increasing the better herbage." A repetition of like experiments in this country could not fail to give valuable results.

In Europe some success has been reached in selecting and cultivating different varieties of Lolium perenne, Dactylis glomerata, and Trifolium pratense.

The field is a promising one for any careful and enthusiastic student. For three years past, I have been studying hundreds of plants of red clover at all seasons and stages of growth. I have plants growing, the seeds of which came from marked plants which varied much from each other. Plants in the fields of red clover vary amazingly in many respects, which influences their value for forage-crops. I believe our fields of red clover to-day contain nearly or quite as great a variety of plants as would a field of Indian corn, if we were to mix in a little seed of all the varieties cultivated in any one state.

Some of our grasses in cultivation are quite variable, notably the fescues, orchard-grass, and perennial rye-grass. It was some time ago observed that alfalfa of California, and lucerne of Europe, were quite different in their capacity to endure dry weather, though they belong to the same species. Different treatment in widely separated countries for many years has wrought a great change.

The subject of changing seed, planting old seed, mixing seed, raising it one year or more in a remote country, and then returning to the starting-point, deserves the attention of careful experimenters.

The late Charles Darwin experimented on the effects of cross and self fertilization of plants, and found that in most cases plants from crossed stock were earlier, hardier, germinated better, and yielded more seeds, than those from seed of self-fertilized plants, while crossing with foreign stock of the same variety is a far greater improvement. The idea is to cross the flowers of a plant with pollen from other plants of the same variety, the seeds of which were raised pure for five or more years in a remote locality, fifty miles or more away.

Mr. Darwin said, "It is a common practice with horticulturists to obtain seeds from another place, having a very different soil, so as to avoid raising plants for a long succession of generations under the same conditions; but, with all the species which freely intercross by the aid of insects or the wind, it would be an incomparably better plan to obtain seeds of the required variety, which had been raised for some generations under as different conditions as possible, and sow them in alternate rows with seeds matured in the old garden. The two stocks would then intercross, with a thorough blending of their whole organizations, and with no loss of purity to the variety; and this would yield far more favorable results than a mere exchange of seeds."

In a word, with plants which may be easily crossed, get some foreign seed of the same sort to mix with your own seeds to raise seeds for ensuing crops.

In 1877 I began some experiments of this kind with Indian corn and with beans, and have since

made others. The advantage shown by crossing of corn over that not crossed was as 151 exceeds 100, and in the case of black wax-beans it was as 236 exceeds 100. Since then similar experiments have several times resulted in showing a large increase in favor of crossing with foreign stock.

In a review of Darwin's book, the *Gardener's chronicle* of England said in 1877, "It is certain that these practical results will be a long time filtering into the minds of those who will eventually profit most by them." The results of my experiments have been widely printed in the agricultural papers of the day, and have been given at numerous farmers' institutes and granges, beginning in the winter of 1877, nearly six years ago; and yet I cannot learn that any other person in this country has attempted similar experiments. I will make one exception, in case of Prof. W. A. Henry of Wisconsin university, who tried the experiment in connection with myself. The results, so far, fully accord with the prophetic statement above quoted from the *Gardener's chronicle*.

In originating new varieties and races, see what has already been done, largely in our own country, in a haphazard way, with strawberries, raspberries, blackberries, gooseberries, and grapes, to say nothing of improvements in ornamental plants.

I need hardly add, that some of the best results, considering the time and means employed, have been obtained by persons who have crossed and hybridized according to some well-devised plan.

Our varieties of fruits in cultivation have become so numerous, that to describe them by the fruit and foliage alone often baffles the skill of the most expert pomologist. In the proceedings of the American pomological society for 1877, 1879, and 1881, I have shown that much help can be obtained by noticing the peculiarities of the flowers of apples and pears. The same is no doubt true, to some extent, with grapes, peaches, gooseberries, and other fruits.

Here is a promising field, full of interest to the botanist, — a field where he may accomplish much to aid the horticulturist, and something to advance science. A new variety of any cultivated fruit can no longer be considered as well described, unless some account be made of the flowers.

It has often been shown that many kinds of insects are beneficial to plants by aiding the fertilization of the flowers. The subject has still about it much that is new. Even Mr. Darwin said he did not suppose that he fully understood all the contrivances for fertilization in any one flower.

If it be true, as my experiments during the past six years help to indicate, that bumble-bees aid in fertilizing red clover, then farmers should try to encourage these interesting insects, even though they be disagreeable companions. Bumble-bees prefer to raise their colonies in old nests of meadow-mice. I mentioned in my last report, that it had been suggested that we should not keep many cats, nor allow hawks, foxes, or dogs to catch these mice; for they make nests which are quite necessary for the bumble-bees, which help fertilize our red clover, and thereby largely increase the yield of seed.

Perhaps it may not be altogether visionary to predict that men will yet engage in raising bumble-bee queens, and sell them to farmers at a fair profit, for starting colonies to improve the yield of clover-seed. We may yet have conventions and societies where the leading object shall be to discuss the merits of different sorts of bumble-bees.

A few years ago experiment-stations in Europe began testing seeds which were offered for sale in the markets. Adulterations were discovered most ingenious in character, harmful in effect, and remarkable in amount.

The more the subject was investigated, the worse it seemed to be. Something of the same sort has been undertaken in this country, showing that even in Michigan some worthless seeds are put on the market. In 1877 and later I tested large numbers of vegetable-seeds purchased of fifteen of our large dealers and growers. Not one of these is free from selling seeds that are worthless. The remedy is not easy. On account of its effect on their advertising, publishers are unwilling to print for their readers the results of these experiments. Only a few people can acquire the information after experiments are made.

In making tests of seeds, we still lack information in regard to the surest and best mode of testing each sort. Here is a good work for some accurate and ingenious scientist to invent new apparatus, learn the proper amount of heat, air, and moisture, for producing the best results, find out whether seeds will thrive best with a constant temperature, or a variable temperature; and learn the best modes of preserving seeds alive from one year to another.

I need hardly mention to intelligent students, that there is an extensive field, a very attractive one, in the study of fungi. The agriculturist who deals with plants, not only wants to know the kinds, but the requirements which are favorable or unfavorable to their development. In the study of effectual remedies against fungi, something has been done; but there is still much demand for more knowledge. Successful experiments in regard to fungi are not likely to be made except by botanists.

I have only glanced at a few points where the biologist can find interesting work which will give threefold returns by advancing science, helping to elevate agriculture, and benefiting our country. There are many experiment-stations in Europe, and some in this country. We hope their number may soon increase, and that liberal and permanent endowments will not be lacking. This association, and all other societies working in the interest of science, can render a great service by doing what they can to encourage experiments in all departments of agriculture. Men can be encouraged to prepare papers, and committees can make reports pertaining to the subject. There is a need of thorough state surveys, solely with a view to the interests of agriculture and kindred subjects. More knowledge of our soils, water, building-materials, plants, timber, injurious fungi, insects, and birds, would return to a state, fivefold the cost of acquiring such information. In brief, then, as one of the humble workers in the interests of agriculture,

I most cordially invite you to turn your attention to some of the problems which vex the husbandman.

PAPERS READ BEFORE SECTION F.

On the use of vaseline to prevent the loss of alcohol from specimen jars.

BY B. G. WILDER AND S. H. GAGE OF ITHACA, N.Y.

IN the absence of the authors of the paper, an abstract of it was read by the secretary of the section, Professor Forbes. Vaseline, when used for the purpose indicated, proves to be an agent unaffected by temperature, and by most chemical substances. It is sparingly soluble in cold alcohol, but wholly soluble in hot alcohol, solidifying on cooling. It can be fitly applied in sealing specimen-jars, and meets many requirements when so used.

A new plan of museum-case.

BY E. S. MORSE OF SALEM, MASS.

THE author described, and exhibited by means of drawings, a new plan of museum-case. He said his observations in the museums of Paris proved the great inferiority of the cases there to those in the United States. He gave, in addition to a detailed plan of a case, some suggestions as to the best method of arranging articles within. Mr. Morse has had the subject of arrangements for museum exhibitions under consideration for several years, and the present plan includes contrivances which he has previously suggested as separate devices.

(*BOTANICAL PAPERS.*)

A supposed poisonous seaweed in the lakes of Minnesota.

BY J. C. ARTHUR OF CHARLES CITY, IO.

IN the summer of last year many cattle and hogs died in the vicinity of Waterville, Minn. Residents in the locality believed that the animals were poisoned by drinking the water of adjoining lakes. There are two lakes of considerable size in the neighborhood: they are free from marsh, and have wooded borders; through them runs a somewhat sluggish river.

At the time of the occurrence, the lakes showed a quantity of dark-green scum on the surface, as well as disseminated through the water. The surface-layers of the scum were in places several inches thick. The scum proved to be a water-weed, having some characteristics like those of the nostoc, but is known to botanists as Rivularia fluitans, and has been described by Cohn, a European naturalist. The plant is spoken of by the author of this paper as a seaweed: he supposed it did not occur in this country elsewhere than in Minnesota, and it is not frequent in Europe.

Last year Mr. Arthur visited the locality of the occurrence, and he repeated his visit this summer; but in each instance too late in the season to examine the scum *in situ*. It appears to be composed of innumerable small round bulbs of a transparent gelatinous substance, which are filled with a dark-green material. After they first begin to be seen on the water, the bulbs increase in number with marvellous rapidity. In about two weeks they begin to decay, and their entire disappearance quickly ensues. These phenomena take place usually in June. As no actual experiments have been made upon animals, the deadly qualities ascribed to the so-called seaweed are as yet a matter of conjecture, though the reported facts tend strongly to strengthen the belief that the plant is poisonous.

Relations of certain forms of algae to disagreeable tastes and odors.

BY W. G. FARLOW OF CAMBRIDGE, MASS.

ALTHOUGH large masses of any decaying vegetation may render water unfit for drinking, the only group of plants to be feared, as far as their effect on the taste and odor is concerned, is the members of the nostoc family, which form floating scums of a bluish-green color. When exposed to a bright sun, especially in shallow water, they are transformed into fetid, repulsive-looking masses of slime, which give to the water the so-called pig-pen odor. The water-supplies of several eastern cities have been thus contaminated, and principally by species of Coelosphaerium, Clathrocystis, and Anabaena. In Minnesota is the representative of a fourth genus, Rivularia, which was first found last year at Waterville by Professor Arthur, and which has been found to be very abundant this year in Lake Minnetonka; and in all probability it occurs in most of the other lakes of this region. The singular fact is, that while unknown elsewhere in this country, the species was found several years ago by Cohn in Silesia, who named it Rivularia fluitans; and it was detected also by Gobi near the Gulf of Riga. It appears also to be very closely related to, if not identical with, an alga abundant in certain parts of England, referred by Harvey and more recently by Philips to Echinella articulata, Ag. This is another illustration of the very wide distribution of the species of the nostoc family, of which we have other recent illustrations in the Nostochopsis lobatus of Wood, first described from the northern states, but which has since been found to be identical with Mazea Rivulariaides subsequently discovered in Brazil, and with Hormactis Quoyi found only at Falmouth, Mass., and the Marianne islands in the Pacific.

There is a strong probability, that in the future Minneapolis may be troubled by the decay of the different nostocs floating in the lakes near the city, where they are very abundant. As far as avoiding trouble from these plants is concerned, undoubtedly river-water is to be preferred to lake-water; but before many years the Mississippi near Minneapolis will be contaminated by sewage, and the water will probably then be obtained from the lakes. If the shallower lakes near the city are used, there can be little doubt that in summer Minneapolis will have the same trouble as that experienced in Boston. Even at greater expense, the water should be brought from large and deep lakes, especially those across which the

winds sweep so as to keep the surface-water roughened.

The spread of epidemic diseases in plants.

BY W. G. FARLOW OF CAMBRIDGE, MASS.

In the case of animals it can be said, that, excepting the diseases attributed to bacteria, they are subject to but few diseases caused by fungi. In the case of plants, however, the greater part of the diseases to which they are subject are caused by parasitic fungi; excepting, of course, the injuries caused by insects, which need hardly be considered in speaking of epidemic diseases. Most of the violent epidemic diseases of plants are caused by fungi of the orders Uredineae, rusts, and Peronosporeae, rots. Fortunately the species of these orders attack only a single species of host, or at most several species closely related botanically; so that, for instance, a rot which would attack the potato would not probably attack the grape, although it might be expected to attack the tomato, which is botanically closely allied to the potato. As might be expected, the most violent epidemics occur during, or just after, unusually wet periods. An epidemic disease spreads either by the dispersion of its spores through the air, or by the transportation of the host-plant on which it is growing; the latter being probably the means by which diseases are carried across large bodies of water, as the Atlantic.

With the introduction of food-plants from Europe to this country come, of course, many of their parasitic diseases. It should be noted, however, that the most violent plant-epidemics of recent times have advanced not from east to west, but from west to east. The best-known case is that of the potato-rot in 1845, and since then the very accurately recorded case of the grape-mildew, Peronospora viticola, has arisen. In the first case, the disease is supposed to have reached Europe from the west coast of South America, by way of the United States. In the latter case, the grape-mould, which is a native of North America, can, as I showed by experiments in 1876, be transferred to the European vine; and it was prophesied that the disease would extend to Europe, and do more harm than with us. The prophesy was very soon fulfilled, as you all know. In the two diseases just mentioned, it is a characteristic of the spores, that in germinating, instead of giving off a filament, they discharge a number of motile zoöspores, each of which is capable of propagating the disease. We have several other species of Peronospora, which produce zoöspores, some of which have apparently crossed from America to Europe; and there are others which, although common in this country, have not yet appeared in Europe, although, following the grape-mould, they may be expected to appear there hereafter. Among these may be mentioned Peronospora Halstedii, which grows on compositæ, and may later be found in Europe on the Jerusalem artichoke. Professor Trelease has recently found a Peronospora on Sicyos in Wisconsin, which resembles the grape-mould in general appearance. The germination of the spores has not yet been observed, but judging by analogy one would expect them to produce zoöspores. It would not be surprising if the Peronospora on Sicyos should also be found hereafter causing a disease of squashes or melons; and its progress eastward might be expected as in the cases previously cited.

The speaker then referred to a modification of the spores sometimes observed in Peronospora. Mr. Earle of Cobden, Ill., collected species on Geranium and Viola, where, instead of the usual branching spore-stalks, the spores were borne on the mycelium close to the breathing-pores; the spores themselves being very much larger than in the common form. A similar monstrosity has been noted by Cornu in the grape-mould. The specimens were collected by Mr. Earle in April, and the speaker suggested that this form of spores might perhaps be an adaptation to the cold and wet weather of spring. The conditions which produce the monstrous forms are worth considering by collectors.

Of the diseases caused by Uredineae which have advanced from west to east, the hollyhock-disease, Puccinia malvacearum, is the best-known instance. Its original home was probably Chili; but it spread through Europe about ten years ago, not, however, by way of this country, as was probably the case with the potato-rot. The diseases produced by fungi of other orders, as Ascomycetes, do not spread with the same rapidity as the rusts and rots. This is shown by the black knot, which is so destructive in this country to plums and some kinds of cherries. It is a native of this country, and is found on most of our wild species of Prunus, especially the choke-cherry, a shrub which has been introduced into many places in Europe. As yet, however, the black knot has not made its appearance in Europe.

The speaker then said that he had just found the grape-mildew growing on the Virginia creeper (Ampelopsis quinquefolia) near Minneapolis. As this plant is closely related to the vine, the occurrence of the mildew might have been expected. In attempting to prevent the spread of the disease to countries where it is now unknown, the discovery is of importance. It is evident, that, to prevent the spread of the disease, the importation of Ampelopsis as well as of grape-vines must be prohibited.

Parallelism of structure of maize and sorghum kernels.

BY E. L. STURTEVANT OF GENEVA, N.Y.

If kernels of flint, pop, sweet, and Tuscarora maize be split parallel to the germ, each race will be seen to present a definite arrangement of structure. Thus, the flint corn presents a germ surrounded by starchy matter, and this in turn by a corneous envelope; in the pop-corn proper, the germ is enclosed in the corneous matter, the starchy matter being absent except as the pop variety intrenches upon the flints; the sweet corn has a similar structure to the pop, but the corneous matter is translucent and wrinkled.

By means of blackboard diagrams, the relative arrangements were exhibited of the 'chit' or germ, the corneous matter, and the starch, in the kernels of the above-named varieties of maize and in sorghum.

These different arrangements are constant, and do not pass into each other. The proportion of these elements is also, in general, constant throughout the development of the kernels. The parallelism which is apparent may be accounted for on the familiar axiom that similar forces acting under like circumstances produce similar results.

Agricultural botany.

BY E. L. STURTEVANT OF GENEVA, N.Y.

IF kitchen-garden plants be closely studied, in many varieties it will be found that selection has differentiated the various natural species in accordance with desired uses. It will be noticed, that, while there is a striking uniformity within varieties in those portions of the plant which have not been selected for improvement, there is a great variation between those portions which have secured attention on account of their uses. Thus, in forty-five varieties of onions growing side by side, the foliage is all similar; yet the bulbs vary in size, color, shape, and habit of formation. The effect of selection concentrated upon visible forms has been to produce and fix changes from the natural plant to such an extent as in cases to mask the original plant, so that historical data must supplement morphological data in order to connect the genetic record. It is clearly evident, that conscious selection is a powerful agency for the changing of form, and by long exercise can overcome the type affixed by nature to a species. In the domesticated plant, the power of intelligence to eliminate, modify, and direct the action of natural laws under a given purpose introduced a new factor to influence plant-growth; and forms designed for uses mask genetic resemblances in those portions of the plant where change means value to man. If these views are correctly stated, then it is seen that an agricultural botany, as an annex to natural botany, is imperatively required for the purpose of furthering classification of domesticated plants; and such an annex must vary in its methods as widely from the methods of the natural botany as cultivated plants vary from feral plants, the key to the motive being in one case the use, while in the other it is the floral organs.

The present condition of the box huckleberry, Vaccinium brachycerum, in Perry county, Pennsylvania.

BY E. W. CLAYPOLE OF NEW BLOOMFIELD, PENN.

THIS was an interesting account of a plant that may become extinct. The discovery of this plant took place over hundred years ago, in Virginia, and it subsequently disappeared until 1846, when it was again discovered by Prof. Spencer F. Baird in Pennsylvania. This peculiar plant exists in Perry county, Penn., and in New Castle county, Md., and in no other known locality in the world. It exists in limited quantities there. Its geographical limits are sharply defined, and never extend, but rather recede, indicating a probability of its extinction.

Relation of root and leaf areas; corn.

BY D. P. PENHALLOW OF MONTREAL, CAN.

IN the absence of the author of the paper, the secretary of the section briefly stated the contents. The paper sets forth the importance of the relations between the aërial and subterranean surfaces of plants, especially in respect to area. The experiments of the author were mainly upon the growth and development of maize, of which he has tabulated careful measurements showing the proportions of areas above and beneath the soil.

Influence of position on seed.

BY E. L. STURTEVANT OF GENEVA, N.Y.

THE 'position' referred to in the title of this paper is that of the individual seeds grown on a spike. The object of experiment was to ascertain the differences of germinating force between seeds from the middle and from the ends of the spike. In trials carried forward at the New-York agricultural experiment-station last winter, it was found, that, for an average of 91 per cent of butt kernels, 88 per cent of central kernels, and 98 per cent of tip kernels, of flint corn, germinated. Other experiments gave the following results: In the butts planted, 79 per cent germinated; of the centres, 84 per cent germinated; and of the tips, 86 per cent germinated. For flint-corn, the tip-kernels have the stronger vegetative power.

Periodicity of Sabbacia angularis.

BY MARY E. MURTFELDT OF ST. LOUIS, MO.

THE attention of the authoress was first drawn to this plant in Missouri. It is a matter of popular belief there, that the plant flowers only once in seven years. Mindful of the story in the Greek Reader, of the *scholasticus* who bought a turtle to ascertain whether it would live a hundred years, Miss Murtfeldt obtained some seed of the Sabbacia, and planted it at once. Seven years have expired since the planting, and now the plant is for the first time in flower. In a brief discussion on this paper, Professor Mason showed reasons for doubting in general the popular notions about periodicity in the flowering of certain plants.

An abnormal orchid, Habenaria hyperborea.

BY W. R. DUDLEY OF ITHACA, N.Y.

THE peculiarities of this orchid, as observed by the author of this paper, consist of the spur characteristic of its generic relations, the smaller size of the plant, the narrowness of the side petals, and the broad spatula-form of the lips of the flower. These changes are apparently in a direction from an irregular to a regular form of flower. The peculiar cases observed, of which mounted specimens were exhibited to the section, may be due to arrested development; but, the author suggested, they possibly indicate a tendency to revert to older and simpler forms. The habitat of this orchid is not invariably in swamps, but also in dry beech-woods, where they are found to bloom much later than in damp regions.

In the discussion of the paper, Prof. E. D. Cope inquired as to the likelihood of a reversion to a variety of non-spurred orchids, an idea which met with a favorable response from the author.

Origin of the flora of the central New-York lake region.

BY W. R. DUDLEY OF ITHACA, N.Y.

The region referred to contains a series of lakes, and is bounded on the west by the Genesee river and on the east by Oneida lake. It is of a low, sandy character, the shores of the lakes having but a slight elevation; but towards the north the country gradually rises to a level of 2,000 feet above the sea. The whole region may be regarded as a series of old eroded valleys, filled with drift deposits, and having occasional lake-basins; its entire characteristics being such as would naturally give rise to a peculiar flora.

Professor Dudley described seven species among a large and varied flora peculiarly localized in this lake-country, the natural or ordinary habitat of which is variously situated to the south-west, west, and north-west. The conclusion he sought to establish was that the waters of the great lakes had formerly flowed through these valleys, and carried with them these several varieties of a widely scattered flora.

The remarks which followed the reading of the essay favored this theory, and pointed especially to the abrupt eastern limit of the species in question.

Development of a dandelion flower.

BY J. M. COULTER OF CRAWFORDSVILLE, IND.

By means of crayon illustrations, the author of this paper displayed the changes which the different parts of a dandelion-flower undergo in the process of growth to full maturity. The main object was to demonstrate the place, and method of origin, of the ovule.

(ZOÖLOGICAL PAPERS.)

Mya arenaria: its changes in pliocene and prehistoric times.

BY E. S. MORSE OF SALEM, MASS.

At a previous meeting of the association, the author showed that the species of shells found in the Indian shell-heaps along the coast of New England differed in their proportionate diameters from the same species living to-day. He pointed out, moreover, that species belonging to similar genera, in the shell-heaps of Japan, had changed in precisely similar ways. It was important to find out, if possible, the cause of these changes. A comparison between the shells of two common species, found north and south of Cape Cod, gave indications that temperature was the inducing cause. The two species selected were Mya arenaria and Venus mercenaria; the former extremely variable, the latter very constant, in its characters. Specimens of these species had been collected in great numbers, both recent and ancient. The following are the indices, of Mya arenaria:—

RECENT.		ANCIENT.	
South of Cape Cod, 61.42.	North of Cape Cod, 61.67.	South of Cape Cod, 62.	North of Cape Cod, 62.78.

of Venus mercenaria:—

RECENT.		ANCIENT.	
South of Cape Cod, 81.01.	North of Cape Cod, 81.10.	South of Cape Cod, 81.51.	North of Cape Cod, 81.81.

Since the waters south of Cape Cod are much warmer than those north of Cape Cod, it was reasonable to suppose that these changes were due to temperature, and that the higher index of the ancient specimens found in the Indian deposits might indicate a colder climate. This supposition receives some support in the fact that a measurement of specimens found in the glacial clays about Portland, Me., and on the Kennebec river in the same state, gave the high index of 66, and a number of Norwich and Red Crag fossils of Mya, which he had the opportunity of measuring at the British museum, had an index of 64; recent Mya from South End, Eng., having the low index of 58.30.

It was interesting to observe, that measurements of Mya in Japan gave, for the southern form, an index of 61.10, and, of a more northern form, 62.50.

In the discussion which followed, Mr. Morse stated that he had made similar observations with regard to other shell-fish.

Some recent discoveries in reference to Phylloxera.

BY C. V. RILEY OF WASHINGTON, D.C.

Every new fact in the life-history of the insects of this genus has an exceptional interest, because of its bearing on the destructive grape-vine Phylloxera. The genus is most largely represented in this country by a number of gall-making species on our different hickories, and the full annual life-cycle of none of them has hitherto been traced. The galls are produced, for the most part, in early spring; the winged females issue therefrom in early summer; and thenceforth, for the remainder of the year, the whereabouts of the insect has been a mystery. The author has for several years endeavored to solve this mystery and at last the stem-mother (the founder of the gall), the winged agamic females (issue of the stem-mother), the eggs (of two sizes) from these winged females, the sexed individuals from these eggs, and the single impregnated egg from the true female, have been traced in several species. There is some evidence, though not yet absolutely conclusive, that this impregnated egg hatches exceptionally the same season; also, of a summer root-inhabiting life. In Phylloxera spinosa, which forms a large roseate somewhat spinose gall on Carya alba, and which has been most closely studied, the impregnated egg is laid in all sorts of crevices upon the twigs and bark and in the old galls, in which last case they fall to the ground.

Up to this time they have remained unhatched, and will in all probability not hatch till next spring, thus corresponding to the 'winter egg' of the grape Phylloxera.

Psephenus Lecontei; the external anatomy of the larva.

BY D. S. KELLICOTT OF BUFFALO, N.Y.

THE species referred to is found in large numbers at the rapids above the falls of Niagara, and is scattered throughout the north-eastern part of North America. The author proposed to supplement the accounts given of it by earlier observers with a record of his own observations, which differed in some respects from those of Dr. LeConte. Several details of anatomical structure were brought to the attention of the members, and illustrated with wood-cuts prepared for the purpose and with specimens mounted in balsam for observation under the microscope.

The Psyllidae of the United States.

BY C. V. RILEY OF WASHINGTON, D.C.

THE Psyllidae, or flea-lice, are rather small homopterous insects, that have remarkable jumping powers. Some of them injure cultivated plants. This is notably true of the Psylla pyri, which blights the buds of pear-trees; and Phylloplecta tripunctata, which crumples the tips of the blackberry. The family has received little attention in the United States, and scarcely any thing has been known of the life-history and development of the species. The paper enumerates 17 described species, four of these being synonymes, and one of them (Psylla pyri) introduced from Europe. They fall into four subfamilies, and represent four genera already characterized, and three new genera, — Brachylivia, Pachypsylla, and Phylloplecta. The new species characterized are Calophya vitreipennis, from Arizona; C. nigripennis, on Rhus copallina; C. flavida, on Rhus glabra; Pachypsylla celtidis-cucurbita, forming galls on Celtis texana; P. c.-pubescens, P. c.-asteriscus, P. c.-umbilicus, and P. c.-vesiculum — all forming galls on leaves of Celtis occidentalis; Blastophysa (nov. gen.) c.-gemma, forming galls on the twigs of the same tree; Ceropsylla (nov. gen.) xyderoxyli, a remarkable form developing in pits on the leaves of Xyderoxylon masticodendron; Trioza sanguinosa, on Pinus australis; T. sonchi, on Sonchus arvensis; and Rhinopsylla Schwarzii, from the cypress-swamps of Florida. The paper records discoveries as to the entomography of the species, and especially those affecting Rhus and Celtis; the latter forming a group peculiar to North America, and the most perfect gall-makers in the family.

The most interesting portion of Professor Riley's paper, to those who are not entomologists, was that where he dwelt on the life-histories and habits of the insects he described. The eggs are attached to leaves by a pedicel, and are somewhat pointed at one end, and often terminate in a filament. The young are broad and flattened, with a fringed margin. They are generally pale, and more or less covered with a flocculent secretion. Those on sumach are dark, and without such flocculence. Those making galls on hackberry have stout spines at the end of the body, by the aid of which they are able to work out of their galls.

Note on Phytoptidae.

BY HERBERT OSBORN OF AMES, IO.

THE Phytoptidae comprise a group of very minute mites, species of which produce galls of various forms on the leaves or twigs of various trees. Recent investigation in Europe has placed the group in a different light from that in which it previously was considered. Their study is rendered difficult by their extreme minuteness, and the care necessary to discover the different stages. One of the most common species produces the little wart-like swellings which occur so abundantly on soft maple leaves. A species on ash leaves produces a swelling which is nearly uniform on the upper and under surfaces of the leaf; while another species on the same tree produces a leafy growth at the end of the twigs, the growth sometimes being inhabited also by cecidomyian larvae. On the elm occurs a large deformed leafy growth, which also contains Phytopti; while still another form of gall occurs on box elder, consisting of a depression on the under surface of the leaf, this depression being filled with a woolly growth, and containing Phytopti.

Notes on the potato-beetle and the Hessian fly for 1883.

BY E. W. CLAYPOLE OF NEW BLOOMFIELD, PENN.

THE author found that only one brood of the potato-beetle appeared last year. This seemed an unusual fact, but no second brood had appeared on the potatoes under his observation. In the present year, no beetles appeared during the early stage of the growth of the plant. This fact had been also noticed in New York and New Jersey. He attributed the cessation in the early part of this year to the same unknown cause which had checked the late brood of last year, and asked the opinions of members in determining the cause. Professor Riley thought the disappearance of the beetle could be attributed to the drought. But Professor Claypole said that in 1881, which was an unusually hot and dry season, the beetles were more numerous than he had ever seen them, and gave him more trouble than ever before or since.

In regard to the Hessian fly, Professor Claypole was of opinion that the insect injured the later wheat much more than the early crop, because the crops that gain full strength are best able to resist the attack. Wheat sown before Sept. 10 escaped the ravages of the fly. The winter wheat being chiefly attacked, the observations on the insect had been directed especially to that crop. Contrary to the opinion of many farmers, Professor Claypole believes there are two broods, one in the autumn, and one in the spring. The insect, it is thought, often killed the stalk in the fall, and then probably died with it.

Professor Riley thought that this class of observations could apply only to certain localities, and that in the southern states the conditions might be entirely changed. Professor Forbes thought there were three distinct broods per year in Illinois. As late as July he had found eggs of a brood already abroad.

The structure of the skull in Diclonius mirabilis, a Laramie dinosaurian.

BY E. D. COPE OF PHILADELPHIA, PENN.

A BLACKBOARD sketch of this dinosaur, as reconstructed by Professor Cope, attracted much attention. The animal existed in the mesozoic age, and is estimated to have been 38 feet long. The skull, which is about four feet in length, is in profile a good deal like that of a goose, but, seen from above, is somewhat like that of a spoonbill. Skulls of this type of reptiles are rarely found, and this one throws much light on the question of the classification of the order. The arrangement of the teeth is very peculiar; and the number is very great, amounting to nearly 2,000. The general form of the animal is that of a gigantic kangaroo. The food evidently consisted of very soft aquatic vegetation.

The trituberculate type of superior molar, and the origin of the quadritubercule.

BY E. D. COPE OF PHILADELPHIA, PENN.

IN the lower eocene, Professor Cope finds all the mammalian molar-teeth to be tritubercule. He has now a complete series of molar-teeth from different mammals in successive horizons, showing all the steps of transition from tritubercule molars of somewhat triangular form and very simple structure, up to the regular quadritubercule tooth, which is defined as of nearly square section and having four tubercles. Man has quadritubercule molars: all the monkeys are similarly equipped. Some of the lemurs have tritubercule teeth. Among lower types, such as marsupials and hedgehogs, about half have the tri- and half the quadri-tubercule development. The insectivora are similarly divided, about half having the old eocene molars and half the modern form. The various steps of development were illustrated by blackboard-drawings.

Two primitive types of Ungulata.

BY E. D. COPE OF PHILADELPHIA, PENN.

THE author announced the discovery of a new mammalian fauna of the eocene, having the following characteristics: 1°. All the fingers and toes are retained; they are plantigrade, each limb having five digital extremities. 2°. The limbs are shorter than usual. 3°. They invariably have a flat astragalus. To the second specification there is one exception, a swimming animal whose hind-limbs were long. One of the discoveries is of a hoof-type animal with carnivorous jaws. It existed in the eocene, and appears to have been of short duration.

In the discussion on this paper, Dr. Dawson stated that some of the plants he had traced in the eocene were well adapted, by the circumstances under which they grew, for supplying food to the creatures described. Professor Cope received this announcement with expressions of pleasure. Thus the new mammal of the old eocene not only bridged the interval between ungulates and carnivores, but also the wider gulf between Dr. Dawson and Professor Cope.

Pharyngeal respiration in the soft-shelled turtle, Aspidonectes spinifer.

BY S. H. GAGE OF ITHACA, N.Y.

DURING the last twenty-five years, respiration in the Chelonia has been investigated with considerable thoroughness, both in this country and in Europe; and at present the chelonian form of respiration is considered to be comparable with that of the mammal, rather than with that of the frog as formerly supposed. While, however, the mechanism of respiration has been very fully investigated, there has been, so far as the author is aware, but one investigator who has considered the organs of respiration in the different groups of turtles. The author showed reasons for believing that a true aquatic respiration, and a true aërial respiration, co-existed in the soft-shelled turtle. It is hoped, that, during the coming year, investigations may be completed which shall determine the exact proportion of the pharyngeal respiration, and the structure of this unusual respiratory organ.

The application of nitrous oxide and air to produce anaesthesia; with clinics on animals in an experimental air-chamber.

BY E. P. HOWLAND OF WASHINGTON, D.C.

THE paper opened with the conclusion of the author that a mixture of nitrous oxide and oxygen, administered in a closed air-chamber, would eventually take the place of ether and chloroform as an anaesthetic for all surgical operations. As ordinarily administered, nitrous oxide cannot be used for prolonged operations, because the blood does not separate oxygen from the gas. Nitrous oxide is expelled from the lungs without change: if it is supplied to them without air or oxygen, death ensues from asphyxia. The author claimed to have administered nitrous oxide for dental and surgical operations in over 30,000 cases. He has found that where unmixed nitrous oxide is used, in the average of cases insensibility is produced in fifty seconds, and recovery from unconsciousness takes place in two minutes. With animals experimented upon, in the average of cases, death ensued within two and a half minutes, where air or oxygen was excluded.

If, at the ordinary pressure of the atmosphere, enough air is mixed with nitrous oxide to support respiration, the mixture fails in producing anaesthesia. But the increase of pressure which can be effected by administration in an air-tight chamber changes the result materially. In such a chamber, with suitable air-pressure, equal parts of air and nitrous oxide breathed from a gas-bag, or a mixture

of 85 parts oxide and 15 parts oxygen, can be breathed for an indefinite time without danger or injury, producing perfect anaesthesia while thoroughly oxygenating the blood. The effect of the pressure of air in the chamber is simply to concentrate the mixture in the gas-bag into smaller space; and, when thus concentrated, the oxide does the work of producing insensibility, while the air or oxygen of the mixture keeps up the vital processes.

The author gave an historical account of the discovery of this method of administration by Paul Bert in 1878, and its subsequent applications. Having used it for many capital operations, Dr. Howland recommends the system unhesitatingly. Some points of its excellence, in addition to those already mentioned, were stated as follows: By augmenting or diminishing the pressure, the degree of anaesthesia may be regulated at will, and with mathematical precision. Therefore there is no danger of any of the accidents incurred through the use of ether or chloroform. When inhalation of nitrous oxide and oxygen is stopped, the patient recovers consciousness in a few seconds, and feels no subsequent discomfort. The action of compressed air on the surgeon and his assistants is not injurious.

After the reading of the paper, the operation of the system was exhibited. The air-chamber in this case was a tight box with glass sides; and the patient was a chicken. Perfect anaesthesia was produced and proved; and then, after the chicken was restored to consciousness, it was again placed in the chamber, and killed by the administration of unmixed nitrous oxide.

Conscious automatism.

BY C. P. HART OF WYOMING, O.

THE author confined his inquiry to the manifestation of conscious automatism in man. The question was whether the centres in the cortex of the brain were essential to the production of automatic functions of this character. Claiming that the destruction of these cortical centres induces complete and permanent motor paralysis, the author drew the conclusion that conscious automatism depends upon the integrity of that portion of the brain in which arise consciousness and volition.

Prof. E. D. Cope, discussing the paper, hinted that the author had raised the question upon mistaken grounds; that conscious automatism, of necessity, originated in the cortical portion of the brain, but by the influences of use and heredity became so far habitual that it is independent of volitional impulses. The question is evidently not one of automatical origination, but of functional independence.

List of other papers.

The following additional papers were read in this section, some of them by title only: A fact bearing upon the evolution of the genus Cypripedium, by *E. S. Bastin;* Leaves of the Gramineae with closed sheaths, by *W. J. Beal;* Observations on Cephalopoda, by *Alpheus Hyatt;* Position of the Compositae in the natural system, by *Joseph F. James.*

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

GOVERNMENT ORGANIZATIONS.

National museum.

Priestley's apparatus. — Priestley's chemical and physical apparatus, now in the possession of his descendants in Northumberland, Penn., has been presented by the latter to the National museum, and will be placed in the collection illustrating the history of science.

STATE INSTITUTIONS.

Iowa weather service, Iowa city.

Weather bulletin for July. — The weather of July, 1883, was very favorable to the crops, being fair, nearly normal in temperature, with an excess of rainfall, and southerly winds prevailing.

The mean temperature of the air was but a little over one degree below normal: last year July was nearly five degrees below normal. The number of hot days has been high, especially during the first and last decade, while the middle decade was cool.

Insolation has been high, because, even during the stormy period, cloudy days were rare, and during the month clear days were numerous. The sun thermometer exceeded 140° on twenty-one days; its highest reading was 161°, on the 23d.

The total rainfall was below normal in southern-central Iowa, from Union to Jasper counties: in the balance of the state it was considerably above normal, averaging about six inches in the north-west and in the south-east, and nine inches in the north-east. The highest rainfall, of fourteen inches, for the month, was measured at Decorah. The number of rainy days averaged ten for the east and north-west, and about six for the balance of the state.

As usual during July, very heavy rains have occurred, but only in the north. The highest rainfall measured on one day was nearly six inches, at Homedale, south of Sibley, in Osceola county, on the 23d; next to this stands Algona, Kossuth county, with over five inches on the same date. But the most notable rain period of the month occurred in north-eastern Iowa, from the 20th to the 23d inclusive, giving very nearly ten inches of rain in Howard and Winnesheik counties.

No tornadoes have occurred, but several squalls have visited parts of Iowa; yet the most destructive of these storms have but touched Iowa. The squall of the 4th started about 5 P.M. in central Iowa, and reached south-eastern Iowa about 9 P.M.: it was not very severe. The squall of the 12th started about 6 P.M. in Black Hawk county, reached the Mississippi in Scott and Clinton counties about 9 P.M., doing much damage by wind and hail : it

continued to spread over central Illinois till about 11 P.M. About noon on the 13th another very severe squall started from south-western Iowa, where considerable damage was done in Fremont and Page counties: the storm increased in fury while spreading over north-western Missouri till about 3 P.M. Another storm of less severity visited north-eastern Missouri and southern Illinois on the evening of the same day. A severe squall with hail reached, on the afternoon of the 18th, into north-western Iowa, coming from Dakota. A southerly squall reached Polk and Jasper counties early on the 16th.

On the whole, the weather during July has been very fine: bright skies, aglow with ripening sunshine, alternated with enriching rains, — summed up in splendid crops of small grain and hay, and excellent pastures, and giving promise of a good crop of corn, for the fall season promises well also.

State university of Kansas, Lawrence.

Weather report for July. — In four of the past fifteen years, the July mean temperature has been lower than in this year; but the July rainfall has been but once exceeded during that period (in 1871).

Mean temperature, 76.18°, which is 2.17° below the July average. The highest temperature was 96.5°, on the 23d; the lowest was 56°, on the 9th: giving a monthly range of 40.5°. The mercury reached or exceeded 90° on seventeen days. Mean temperature at 7 A.M., 71.27°; at 2 P.M., 85.71°; at 9 P.M., 73.90°.

Rainfall, 7.23 inches, which is 2.94 inches above the July average. Rain fell in measurable quantities on nine days. There were five thunder-showers. The rain of the 30th yielded 3.10 inches. The entire rainfall of the seven months of 1883, now completed, has been 29.03 inches, which is 7.99 inches above the average for the corresponding period of the preceding fifteen years, and is 1.43 inches above the total rainfall of the year 1882.

Mean cloudiness, 39.46% of the sky, the month being 1.89% cloudier than the average. Number of clear days (less than one-third cloudy), 18; half-clear (from one to two thirds cloudy), 7; cloudy (more than two-thirds), 6. There were three entirely clear days, and three entirely cloudy. Mean cloudiness at 7 A.M., 38.39%; at 2 P.M., 45.48%; at 9 P.M., 34.52%.

Wind: S.W., 39 times; N.E., 15 times; N.W., 12 times; N., 9 times; S., 7 times; W., 5 times; S.E., 5 times; E., once. The entire distance travelled by the wind was 10,901 miles, which is 2,229 miles above the July average. This gives a mean daily velocity of 351.64 miles, and a mean hourly velocity of 14.65 miles. The highest velocity was 40 miles an hour, from 1.30 to 2 A.M. on the 12th.

Mean height of barometer, 29.086 inches; at 7 A.M., 29.111 inches; at 2 P.M., 29.071 inches; at 9 P.M., 29.078 inches; maximum, 29.381 inches, on the 18th; minimum, 28.679 inches, on the 11th; monthly range, 0.702 inch.

Relative humidity: mean for the month, 71.4; at 7 A.M., 80.3; at 2 P.M., 54.7; at 9 P.M., 79.1; greatest, 97, on the 31st; least, 20, on the 2d. There was no fog.

NOTES AND NEWS.

Circumstances were not favorable to the production of remarkable essays at the recent meeting of the American association. The attendance was not large. The officers of the meeting, and especially those who had to make addresses, could scarcely be expected to produce elaborate papers in addition to their other labors. As the number of addresses per meeting has increased, we may observe more readily some of the effects of the system that demands them. The most evident result is, that usually, where we gain one good address, we lose two or three good papers.

The distance of the meeting from their homes affected especially members of sections A, B, C, and D, devoted to the exact sciences. Perhaps it affected the quality as well as the number of their papers. There were not many from the east to present essays, though quite as many as could have reasonably been expected; but there were scarcely any from the locality of the meeting and its neighborhood. Local interest, both as to authors and hearers, was of course deficient. In short, there was nothing remarkable in those sections to spur production, and the product was not remarkable. It was good, but not great.

Some of the papers seem to have lost their way among the sections; a paper that was chiefly botanical having gone before the chemists, and the paleontological papers being divided between biology and geology. In some cases the affinities of authors rather than of subjects may have been consulted, though probably the discrepancy was mostly created in efforts to equalize the amount of work in the different sections.

During the progress of the meeting, it being found that botanists were present in unusual numbers, a botanical club was formed. The immediate object was the organization of botanical excursions. An ultimate object is to arrange for preparing a petition to memorialize congress respecting differences between the rulings of the post-office department as to the sending of plants by mail at home and abroad. The organization of the club was somewhat informal. Prof. W. J. Beal of Lansing, Mich., was appointed president, and John M. Coulter of Crawfordsville, Ind., secretary. The roll was signed by twenty-five botanists who were present at the first session of the club, and their number was increased before the meeting of the association adjourned.

We have before alluded to the singular want of executive ability, or of co-ordination in achieving results, which marred the work of the local committee. That continued throughout the meeting, with many embarrassing results. We again refer to it, not to find fault anew, but to mention that the committeemen themselves acknowledged their blunders most heartily in their farewell speeches, and that their kind intentions were manifest throughout.

— Students of meteorology will be interested in a paper lately read by M. Faye before the French academy of sciences on the whirlwinds of sand observed by Col. Prejevalsky in central Asia. M. Faye believes that such sand-storms, like those of Mexico, India,

and the Sahara, have the same origin and mechanical action as the tornadoes of the United States and all water-spouts. They are vertical spiral movements, moving horizontally and nearly in a straight line.

—The operations of France in the region of Annam have naturally excited great interest in the geography and ethnography, statistics and commerce, of Annam. A crowd of publications of all sorts are constantly appearing. References of the briefest sort to some of the more notable may be of interest to those who ignore the political side of the question. J. Gaultier publishes for Mallard-Cressin a chart of the region on the scale of 1: 850,000. This is stated to be on the largest scale of any of the maps of this region, and as perfect as the state of knowledge will admit. Another map by Henri Mager, though smaller, is very carefully executed, and includes a plan of the fortress of Hanoi. The oriental studies of the author have enabled him to unify and correct the nomenclature in a satisfactory manner. Romanet du Caillaud has published a long memoir on the protectorate of France over Annam, and the relations between the latter state and China, in the quarterly bulletin of the Société de géographie.

—The enterprise of Johns Hopkins university is shown by the publication of one of its circulars in midsummer, filled with scientific notes in mathematics, physics, biology, and philology. They are all abstracts of papers read before the different active associations in the university, and in most cases will probably be published in full elsewhere. The circular also reprints, from the Royal society's proceedings, the abstract of Dr. Martin's Croonian lecture; and, from the London *Times*, an account of the eclipse observations of May 6, to which Dr. Hastings appends a brief note, pointing out one mistake made by the writer. A list of mathematical models belonging to the university, and of works in the Assyrian and other oriental languages found in the Peabody institute, are also given.

—The following appointments to fellowships in science in Johns Hopkins university are published: In mathematics, G. Bissing and E. W. Davis of Baltimore, and A. L. Daniels of Kendallville, Ind.; in physics, Gustav A. Liebig, jun., of Baltimore, and Charles A. Perkins of Ware, Mass.; in chemistry, D T. Day of Baltimore, J. R. Duggan of Macon, Ga., and E. H. Keiser of Allentown, Penn.; in biology, W. H. Howell and L. T. Stevens of Baltimore.

—Müller's record of the literature of pollination and dissemination for 1880–81 has recently appeared in Just's *Jahresbericht*, containing abstracts of one hundred and forty-nine papers, with many useful items, both critical and supplementary, by the able reviewer. Though these records are very useful when they reach us, their value would be much increased if it were possible to present them to the public more promptly after their preparation. As it is, they are usually two or three years in appearing.

—*Nature* states that the Dutch government have decided not to grant the sum of thirty thousand guilders, which Baron Nordenskiöld claims as the discoverer of the north-east passage. The decision is founded on the motive which led the States-general, in 1596, to offer this award; viz., to find a passage of *commercial* value to the nation. Baron Nordenskiöld having, however, discovered what may be termed a purely *scientific* one, the award, it is argued, has not been earned. As several reasons have been advanced for this claim made by the gallant Swedish explorer, we do not think we err, says *Nature*, when we assert that it was his intention to have expended the sum in the interest of science; viz., on an expedition to the arctic regions.

—George Mantoux has just edited a volume containing the letters and journals of La Pérouse, on his celebrated and unfortunate voyage around the world; preceded by a memoir of that officer, who was last heard from at Botany Bay, and, with his entire party, was wrecked on one of the South Sea Islands, where the survivors were murdered by the natives. It forms one of the *Bibliothèque d'aventures et de voyages* issued by Dreyfous of Paris.

—A Yokohama paper states that Mr. John Milne, whose researches on earthquakes, as explained by him to the British association at Southampton, have excited great interest in scientific circles, and who has since returned to his duties in Japan, has applied to the Japanese authorities to establish an observatory, in order that he may be able to thoroughly investigate underground phenomena. He has sent the authorities a long treatise upon the earthquakes of Japan.

—The *London daily news* says that the Darwin memorial fund has risen to £3,300. Among the most interesting of the sums that the treasurer has received is a cheque for £94.4, collected in Finland.

— The next number of the *Journal* of the Cincinnati society of natural history will contain an illustrated paper by Professor Mickleborough, upon a specimen found by Mr. D. A. McCord of Oxford, O., which has been creating much interest among the paleontologists of Cincinnati and vicinity. It is a small slab of limestone showing on one side the shell of an Asaphus, and on the other the legs of the animal. Fortunately, the rock was split in such a way as to show both the legs and their cast. The characters of the ambulatory appendages of the trilobite are finely shown, and confirm in a remarkable manner the discoveries of Mr. Walcott, who several years since established beyond a doubt the existence of legs in specimens of Calymene.

—The bodies of Professor Palmer, and his companions Capt. Gill and Lieut. Carrington, assassinated by the Bedouin, have been discovered by Capt. Warren, and transported to England, where it is anticipated they will find a resting-place in St. Paul's cathedral.

—Mr. Charles Depérais read a paper before *l'Institute royal d'encouragement de Naples*, April 5, in which he advocated the embalming of bodies by boiling them in a solution of chloride of calcium, and then in a solution of sulphate of soda.

—The government of Ontario has published for the Entomological society of that province a general index to the thirteen annual reports upon injurious insects which the society has made to the commis-

sioner of agriculture. The index is prepared by William Baynes-Reed, and consists of a serial and a classified list of illustrations, and a general index to the text. It appears to be prepared and printed carefully.

— The death of the famous M'tesa, King of Uganda and baiter of missionaries, is announced.

— The following papers were prepared during the past year by members of the Lawrence scientific school, Harvard university, under the supervision of Dr. E. L. Mark in the embryological laboratory at the Museum of comparative zoölogy: —

On the development of Oecanthus, and its parasite Teleas, by Howard Ayers of Fort Smith, Ark.; on the development of the posterior fissure of the spinal cord, and the reduction of the central canal, in the pig, by William Barnes of Decatur, Ill.; notes on the development of Phryganidae, by William Patten of Watertown, Mass.; the relation of the external meatus, tympanum, and eustachian tube, to the first visceral cleft, by Albert H. Tuttle of Dorchester, Mass.

The papers by Mr. Ayers and Mr. Patten have been awarded respectively the first and one of the second Walker prizes by the Boston society of natural history, as already stated in these columns. All are to be published in the course of a few weeks.

— The eighth annual report of the Buffalo microscopical club shows a membership of forty-six, — a gain of fifteen during the year. The average attendance at the monthly meetings is stated to have been about twenty-five, — certainly a very large percentage.

— Prof. D. P. Penhallow, having resigned his connection with the experiment department of Houghton farm as botanist and chemist, has accepted the lectureship of botany at McGill university.

— Messrs. Allen, Coues, and Brewster sign a call for a convention of American ornithologists, to be held in New-York City, beginning on Sept. 26, 1883, for the purpose of founding an American ornithologists' union, upon a basis similar to that of the 'British ornithologists' union.' The object of the union will be the promotion of social and scientific intercourse between American ornithologists, and their co-operation in whatever may tend to the advancement of ornithology in North America. A special object, which it is expected will at once engage the attention of the union, will be the revision of the current lists of North-American birds, to the end of adopting a uniform system of classification and nomenclature, based on the views of a majority of the union, and carrying the authority of the union.

It is proposed to hold meetings at least annually, at such times and places as may be hereafter determined, for the reading of papers, and the discussion of such matters as may be brought before the union. Those who attend the first meeting will be considered *ipso facto* founders. Active and corresponding members may be elected in due course after organization of the union, under such rules as may be established for increase of membership. Details of organization will be considered at the first meeting.

— 'The books of science' is the title of a work announced by Leypoldt as in preparation by William C. Lane of Harvard college library. It is to be an annotated catalogue of the most trustworthy works for the study chiefly of the physical and mathematical sciences. From what we know of the compiler and of the manuscript, a portion of which we have examined, we may confidently predict a very useful work.

— In his address before the American forestry congress last year at Cincinnati, recently printed in the *American journal of forestry*, Prof. F. L. Harvey gives a catalogue of the forest-trees of Arkansas, of which he enumerates a hundred and twenty-nine indigenous species. According to his summary, Arkansas is remarkable for its extensive belts of pine, for the area of hard-wood growth, and for the number of species usually classed as shrubs, which here attain the dimensions of trees. More than half the species belong to the six orders Magnoliaceae, Rosaceae, Urticaceae, Gleaceae, Juglandaceae, and Cupuliferae. Professor Harvey believes that physical conditions, rather than geological horizon, affect the specific character of the vegetation in Arkansas, where the north-western part of the state is upland and paleozoic, and the remainder lowland and of more recent date.

RECENT BOOKS AND PAMPHLETS.

Paluzie, F. La historia natural explicada á los niños, según las clasificaciones de Couvier. Madrid, *Perdiguero*, 1883. 160 p. 8°.

Registro general de la industria española, con una sección extrangera, en que figuran las fábricas y establecimientos industriales más importantes de los diversos paises de Europa y América, y agenda del industrial, continuación del Almanaque publicado desde 1875, por la *Gaceta industrial*. Año primero (1881-82). Madrid, *Tello*, 1882. 238 p. 4°.

Ritsema Bos, J. Insektenschade op bouwen Weiland. Handleiding voor de kennis van de kleine vijanden van akker- en weidebouw. Groningen, 1883. 216 p. 8°.

Roiti, A. Elementi di fisica. Firenze, 1883. 12+356 p. 16°.

Roura, J. Tratado sobre los vinos, su destilación y acetos. Madrid, *Perdiguero*, 1883. 113 p. 8°.

Sack, J. Die verkehrs-telegraphie der gegenwart. Wien, 1883 (Elektro-techn. bibl., v.). 272 p., illustr. 8°.

Sieiro y González, J. Principios de psicología ó anthropología psiquica, lógica y ética. (Orense), *impr. Ramos*, 1882. 319 p. 8°.

Smith, Ch. Conic sections. London, 1883. 8°.

Smith, J. M. The Hades of Ardenne, a visit to the caves of Han. Described and illustrated by the T. T. Club. London, 1883. illustr. 8°.

Sonklar v. Innstaedten, C. Von den ueberschwemmungen, enthaltend die ueberschwemmungen im allgemeinen, chronik der ueberschwemmungen und mittel der abwehr. Wien, 1883. 151 p. 8°.

Sundman, G., and **Reuter**, O. M. The fishes of Finland (and Sweden). pt. i. Helsingfors, 1883. 9 p., 3 col. pl. f°. Will contain about 30 parts.

Tobler, A. Die elektrischen uhren und die feuerwehr-telegraphie. Wien, 1883 (Elektro-techn. bibl., xiii.). 240 p., illustr. 8°.

Ungarn, Geologische special karte von. Herausgegeben von der k. ungarischer geologischen reichsanstalt. Budapest, 1883.

Walras, L. Théorie mathématique de la richesse sociale. Leipzig, 1883. 256 p., 6 pl. 8°.

Wittstein, G. C. Handwörterbuch der pharmakognosie des pflanzenreichs. Breslau, 1883. 994 p. gr. 8°.

Zacharias, J. Die elektrischen leitungen und ihre anlage. Wien, 1883. (Elektro-techn. bibl., xvi.) 272 p., illustr. 8°.